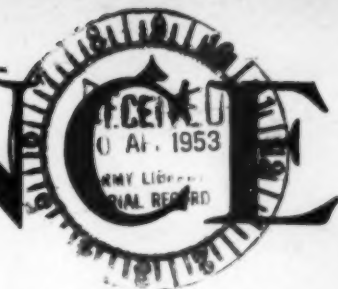


SCIENCE



APRIL 17, 1953

VOLUME 117

NUMBER 3042

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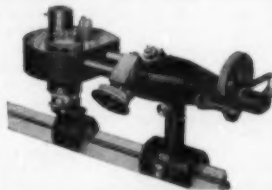
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On the Astin Dismissal

CERTAIN ASPECTS of the official dismissal of Allen V. Astin from his post as Director of the Bureau of Standards have confused and dismayed the scientists of this country, and have raised serious issues. From the published statements there is reason to fear that considerations have entered other than those of scientific and administrative competence. Great concern has been evinced over the possibility that scientific objectivity has been subjected to political interference.

Science and government have become so thoroughly intermixed in the past 15 years that it is hard to know where the one begins and the other leaves off. How to manage this marriage of necessity has become of concern recently to many thoughtful men. Reasoned consideration of these problems has not been aided by the abrupt action taken by Secretary Weeks. The National Bureau of Standards, center of much basic research as well as military and atomic development, has long served as consultant and advisor to other Government agencies on technical matters within its purview. In this capacity it tested a commercial preparation, AD-X2, allegedly effective in prolonging the life of storage batteries. The Bureau found the preparation without practical effectiveness and so reported. Other Government agencies acted on these findings and took steps which the manufacturer regarded as prejudicial and damaging to his interests.

His protests led the Senate Small Business Committee to request an independent study by M.I.T. The M.I.T. tests, reported to the Senate Committee, showed some effect of AD-X2 on batteries under laboratory conditions, but no conclusions were drawn regarding the practical effectiveness of the material. Their results were interpreted, however, by Keith J. Laidler, a consultant to the Committee, as contradicting the N.B.S. conclusions.

The technical controversy thus precipitated is hardly novel. Extrapolation from laboratory tests to field conditions is notoriously beset with pitfalls, and tests conducted in different laboratories by different procedures often disagree. Given the desire, such controversies can nearly always be resolved by new tests under mutually agreed laboratory or field conditions. What is novel, at least in the United States, is to have a high Government

official step into a technical disagreement and seek to resolve it by dismissal of the director of the Government laboratory involved. Particularly novel—and alarming—is the official's justification of his action, at least in part, on the ground that the director of the laboratory had not given sufficient attention to the "play of the market place." In the marriage of science and government, few scientists would assert that science must be the dominant controlling partner. But even fewer will accept dominance and control by government officials over objective investigation and its results. It is hard to see how confidence and good working relations can be restored unless the Astin dismissal promptly has the fullest and most impartial public investigation.

It is clearly of importance to our country and to science that the directorship of our national physical laboratory be a post which will attract scientific leadership of the highest type. To help assure this is the clear duty of the great national organizations of science. In such situations, it is seldom that the most effective approach is through public charges or formal protests. In the present situation several of the great national scientific organizations have in fact been active. In particular, the membership of the AAAS should know that its present ranking officer, the Chairman of its Board of Directors, has had several conferences in Washington, including an extended talk with the Secretary of Commerce. The Association officers are also in the closest touch with the other groups concerned with this problem.

Although some damage has been done which cannot be undone, nevertheless it seems reasonably clear that understandings now exist and that steps are being taken which will go far to avoid further trouble. In particular, the Visiting Committee to the Secretary of Commerce for the Bureau has now been asked to meet with the Secretary to discuss the efficiency of the scientific work of the Bureau; and a committee has been established to study the present situation in its general administrative aspects.

In this whole difficulty it is important to recognize that the battery additive matter can only be regarded as minor. The important issue here is the fact that the independence of the scientist in his findings has been challenged, that a gross injustice has been done, and that scientific work in the government has been placed in jeopardy.

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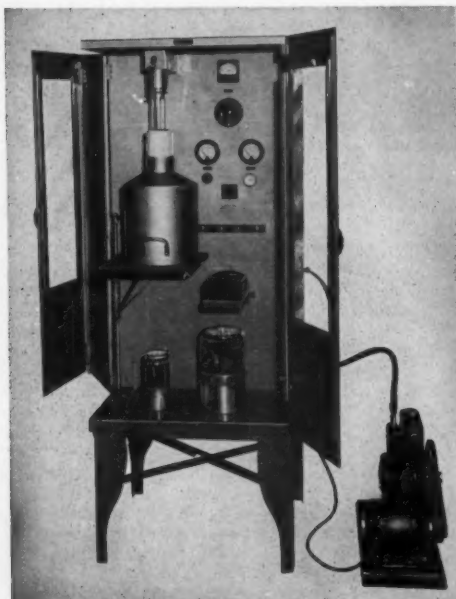
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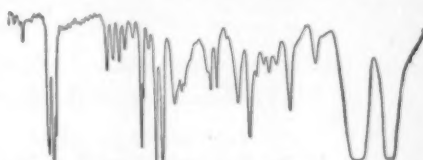
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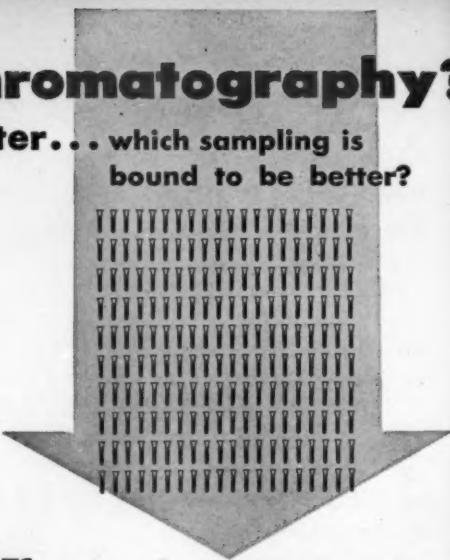
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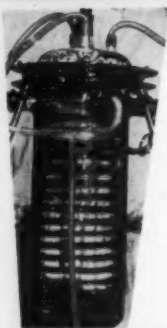
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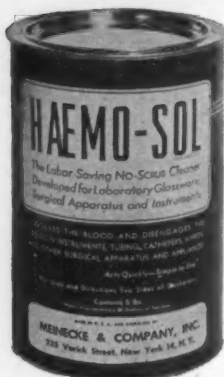
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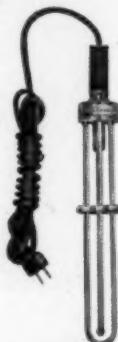
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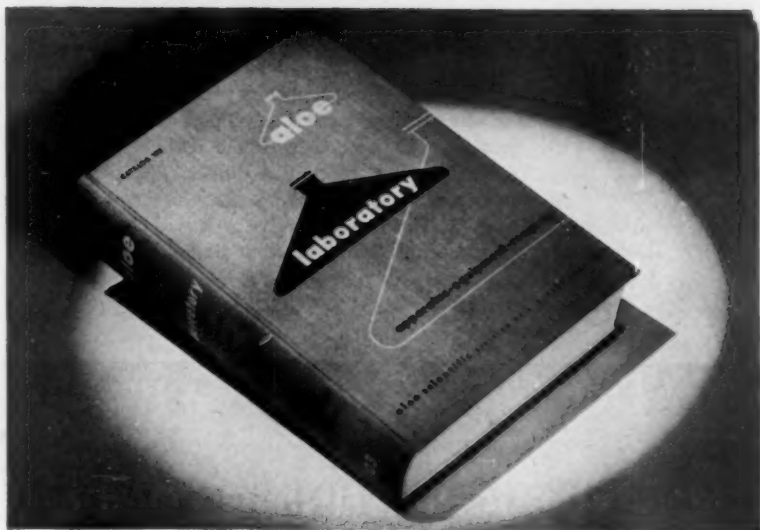
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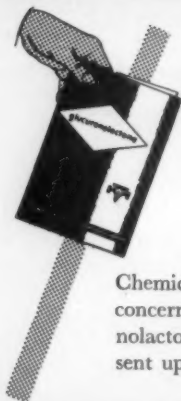
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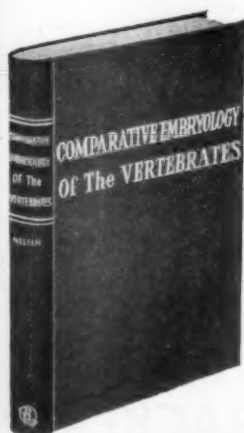
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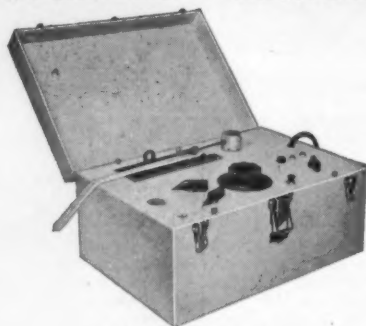
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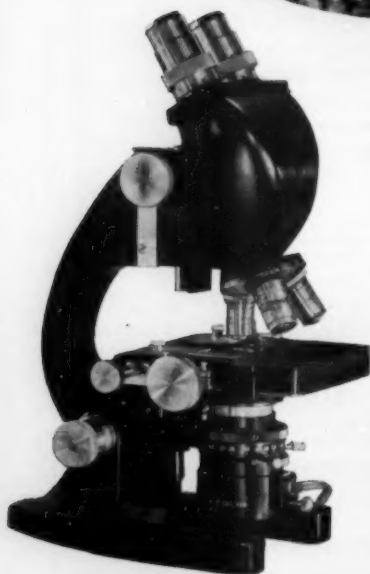
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Editorial Department, Harper's Magazine, New York

ONE OF THE PARADOXES of the present period is that science, while relaxing metaphysical claims, has extended social ones. It is not unusual for the modern American scientist to find himself making demands on society that would formerly have been thought unnecessary to make—for financial support, for freedom from interference, for understanding of scientific aspirations and techniques on the part of the public. In protecting his own self-interest as a scientist he is compelled to enter arenas of value judgment from which science, as a philosophy, has progressively withdrawn itself. The principles on which claims might be based are no longer, if they ever were, generally accepted; and on every hand are signs of antiscientific sentiment. Deprived of the protective devices that operate within the scientific fold, the scientist is confronted with the problem of how to conduct himself in the fields of nonscientific behavior, of which poetry and politics, one for individuals and the other for society as a whole, are two extremes.

If the need were merely for a better press, for "selling science to the people," then the AAAS could hire a competent public-relations counsel and leave the matter in qualified hands. Unfortunately it is nothing of the kind. The problem of Anti-science, though it may be a subdivision of the larger one of Anti-intellect, is not amenable to the manipulation of opinions. Palliative measures—like encouragements for more and better teaching and popularization of science—do not alter the conditions that have brought it into existence. They are, in fact, likely to be self-defeating and to alienate as often as they attract. The trouble is not too little publicity but too much, not its failure but its success. Scientists as a class, like nearly every other in contemporary America, are prone to exaggerate the degree to which they are persecuted, ignoring the existence of their own prestige in order to visualize themselves as underdogs. The vast admiration that science actually enjoys is not only more widely shared than the antipathies against it, it is partly responsible for them. At least one source of Anti-science lies in the deepening absorption of science by society, a further interpenetration of one by the other than had earlier existed. Anti-science is in many respects the friction that this process inevitably generates.

I do not mean to minimize the difficulties that scientists face, particularly since I write as an avowed generalist whose own problem is of a different, if not opposite, nature. But I write also in the conviction that they cannot be reduced without lay participation,

that they involve propositions on which nonscientists have a responsibility to speak, and that science is much too important (to paraphrase Clemenceau) to be left to scientists alone. The limitations on a layman's prerogative are obvious and need neither be elaborated nor excused. This paper has no other justification than to offer scientists an outsider's view of their predicament. It is a sympathetic one, although it may not invariably appear so. The writer is fully aware how precarious his position is, but would rather take his chances than apologize for it, in the belief that science will be better served by friendly criticism than it is at present by its uncritical friends. Only from the outside, in any event, can the claims that science is now making beyond its proper sphere be validated.

II

The situation would be simpler to describe if those claims reached farther and were more vigorously engaged. "From nucleonics to sociology," writes the physiologist Ralph W. Gerard, "there exists in principle a continuum." Why stop at sociology? If there exists a definable boundary at which the orders of knowledge become qualitatively different, it must lie on the scale well beyond the region where the human personality begins overtly to intrude itself. If sociology is part of the scientific continuum, if only in principle, then so also must be the humanistic studies of behavior, which draw on poetry and politics, among other resources, for their factual evidence. Admittedly this is a wavering line of controversy, but it is the one from which science in the past quarter century has conducted a metaphysical retreat. The brave assertions of the behaviorists—like John B. Watson's "We need nothing to explain behavior but the ordinary laws of physics and chemistry"—are no longer to be heard except in faint echoes among the Social Physicists. During the same period in which science has made its longest forward strides in both performance and public esteem, it has reduced its aims and shortened its philosophical reach.

To be sure, it would be unreasonable to expect science to be permanently associated with optimism, even about itself. An increase in knowledge, as we frequently are reminded, is also an increase in ignorance. Only the innovators of scientific method like Descartes and Bacon could assume that, if it were widely and truly applied, all conceivable questions about the cosmos would be answered in from six to sixty years. Yet it is curious that scientific self-confidence should fluctuate as it does, from one generation to the next, especially in its relationship to competing and con-

flitting doctrines. Nowhere is this more apparent than in the rhythm of changing tensions between science and religion, which at the moment have fallen slack and seem not to be of pressing importance. No one thinks it strange that the Pope should enunciate a doctrine of creation timed to an expanding universe or that a scientist of the stature of E. U. Condon should speak of the "truths of science" and the "truths of religion" as though they were complementary. Such circumspection must be both a puzzle and a relief to mature scientists now at work who can remember the Scopes trial, or who may indeed have grown up on Andrew D. White's *History of the Warfare of Science with Theology in Christendom*. Confronted with the current revival of religiosity among intellectuals, they must be tantalized by the ironical thought that science has won all the battles but lost the war.

Obviously such is not the case. A great deal of confidence in science—as, for that matter, of science in itself—is dormant, beneath the surface, taken for granted, and no less operative because it is unobtrusive. Despairing scientists in search of a more realistic impression of what has happened should perhaps observe more closely the morale of their opponents, who are compelled to admit, like the British theologian C. S. Lewis, that the war is over and that a materialist faith is everywhere triumphant. To the Anti-scientist, also thinking himself an underdog, this era seems saturated with a pragmatic disregard of supernatural sanctions. The easy-going empiricism of everyday life, in that sense, is both an index of science's success and a potential source of its strength, however little bearing it has on scientific philosophy at a sophisticated level. The point at issue here, however, is not the score of an intangible contest between ideologies. It is apparent that in science's house are many mansions, that there are many ways of "believing" in it or not, and that its forward progress about its main business does not depend directly on the regard in which it is held. Science has at best a negative or indirect effect on numerous currents in the climate of opinion, including some that have an effect on the intellectual reputability and what might be called the "political" status of science. Yet one could fairly deduce, I think, if only from the contemporary preoccupation of scientists with proselytizing and with self-protection, that in the near future no *status quo* for science, in its nonscientific situations, can be maintained.

III

For the purposes of the paragraphs which follow, it will be assumed that advance and retreat are the only alternative tactics and that of the two the former is preferred. Perhaps it is debatable whether science can ever serve as a universal organizing principle for those who are unable to apprehend its subtleties. Years of disciplined study, as Ernest Nagel has argued, are required for understanding the conceptions now employed at the outer edges of scientific

advance. Perhaps the injunction of James B. Conant, that the uninformed public refrain from speculation of any kind about a subject (nuclear weapons, in this instance) on which essential facts must be concealed, reflects a typical pattern of divided knowledge to which we must become permanently reconciled. I should prefer not to think so, and not to accept the specialist's point of view, though it is valid in itself, without a generalist's modification. "Great scientific advances are not now," as Charles Singer writes, "nor have they ever been, of their own nature specially difficult of comprehension. . . . If those men of science be right who assume as inevitable their own unintelligibility to a public all too ready to accept this assumption, then is the outlook of our age gloomy indeed." Rather, let us assume that science has no theoretical limits, either of applicability or acceptance.

What, then, are the obstacles to advance? Many of them come quickly to mind—mistrust and resentment of a morally neutral position, of a specialist outside his specialty, of allegiance to principles above national jurisdictions, of disturbing opinions for which no individual can be held accountable, and so on. Leaving aside those persons who oppose science for simple doctrinaire or unconsidered reasons, there is clearly a complex of many attitudes involved for the thousands of educated and rational people who, to the seeming detriment of science, continue to hold nonscientific beliefs. Since their convictions have survived as much as half a century of concerted attack, it is unlikely that they will be converted overnight, or by a television program on recent developments in marine biology. Many of them are apparently satisfied with a working allegiance to technology, rather than to science, as the fount of material welfare. If forced to choose between cumulative and noncumulative types of knowledge, many will reject the former in favor of the arts and letters that make life worth living and remain alive while science goes out of date. If science is to make any significant inroads on Antiscience in our lifetime, these are the people who must be convinced that science has music and color and poetry of its own.

I have not contrasted poetry with science in the naïve belief that all scientists are by definition insensitive to poetry. Yet disparagement of intuition in any form is a part of the scientific tradition, even if it is not universal or compulsory, or limited to scientists, for that matter. There is a certain thread of consistency in the response of scientists and poets to one another, from Bacon onward (how anyone, incidentally, who knew Bacon's low opinion of poetry could think he wrote the plays of Shakespeare is one of the real mysteries of nonscientific behavior). Newton was not alone among scientists in thinking poetry "a kind of ingenious nonsense," nor Blake among poets in calling science "the tree of death." Over a period of centuries, it is also a one-sided relationship, for the most part, with poets making the greater attempt to accommodate science than the other way

around. They were more interested in Newton than he was in them, as in our own day T. S. Eliot has encompassed more science than science has encompassed him. While poets have struggled to preserve a place for value in a world of fact, few scientists have had to concern themselves with finding a place for fact in a world of value.

We might be better off today if more of them had. Many readers of this journal are presumably familiar with the deplorable state of isolation from its audience into which the poetic art is generally thought to have fallen. Many of them may be surprised to know, however, that several critics hold modern science responsible for this. The two most recent scholarly books on the subject—Douglas Bush's *Science and English Poetry* and Hyatt Howe Waggoner's *The Heel of Elohim: Science and Values in Modern American Poetry*—share the view that all modern poetry has been conditioned by science, even when seeming to react adversely, into avoiding clear and logical statement in favor of intentional complexity, ellipsis, and ambiguity. Mr. Waggoner puts it thus:

Now if the observational and experimental techniques of science really constitute the only valid approaches to truth . . . then it follows that poetry, if it is to seem significant, should . . . appeal to the sensibility (defined as primarily if not wholly emotional) but not to reason. . . . It should, indeed it must, be this kind of poetry to be taken seriously: for we cannot take it seriously if it is only poor science (it is clearly very bad science). . . . It must be thus, then, because in a world in which a divorce has been arranged between fact and value, poetry, which cannot compete with science in handling the kind of facts that science handles—and these are thought to be the only facts there are—poetry must keep strictly to the realm of value and leave the other realm to science.

"So much the better!" might be the reply of scientists who hold that science has no other responsibility than the untrammelled pursuit of its own ends. Yet if they choose to live and wish to be effective in a world in which poetry is also a fact, in which emotions undeniably exist and operate, then their position is untenable. Since it is little better than verbal and essentially false, the distinction between fact and value crumbles at the touch. In a strictly observational fashion, it is impossible to find values that are free of fact or facts that are free of value: the notion that one may do so is merely a convenience, and it becomes increasingly less convenient the more we suffer its arbitrary and obnoxious consequences. If science builds its future on these shifting sands it will not only build poorly, it will invite the ultimate undermining of the structure by the forces thus removed from scientific sustenance and restraint.

Science, at its own peril, may continue to treat the intuitions of which poetry is the purest product as an unrelated avenue of experience. The materials on which the poetic intuition works are no less factual because they are not statistically handled, nor is the intuitive process less accurate because it is rapid and deals with probabilities, using a mental shorthand in

which intermediary steps may not be consciously performed. Intuition is commonly called upon to manage an unlimited number of variables—the connotations, say, that a given word in a given poem will have for all possible readers—and to produce an approximate answer instantaneously. Intuitive conclusions may often be wrong, but not because they are intuitive or because any other method could have produced better ones. I trust these words will not be misinterpreted as a request that science scuttle mathematics and experiment forthwith, to rely henceforward on hunches and inspired guesswork—though much fruitful scientific work has had an assist from intuition in the past, and will presumably continue to enjoy its unpredictable and irreplaceable aid. Nothing need be abandoned that is now possible, nothing need be sacrificed that has proved its worth in any category, in lowering the artificial barrier that separates science from the proper studies of mankind. I do not presume that the sciences of the nonscientific which eventually result will be exact facsimiles of mid-twentieth-century models—or that they will need nothing more than "the ordinary laws of physics and chemistry"—but I do presume that they will be scientific in the best sense, in the traditional sense, which is science's only permanent legacy.

IV

Already there is a mounting body of evidence to suggest what the outcome will be if science hesitates to extend itself and withdraws into the security of only those "facts" that can be weighed and measured, or entered in the coding devices of electronic computers. In a mass-educated society people crave enlightenment, and, when they do not receive it from accredited sources, they will search elsewhere. Much of the faddist and crank behavior that perplexes and annoys the scientific community, often giving it the sense of being surrounded by a sea of irrationality, belongs in a grouping that might be titled "vacuum phenomena." Where an admiring but overdramatized picture of psychiatry is more widely accessible than reputable treatment, the result is dianetics. Where there is a pervasive sense of inadequate diet but only sporadic efforts to improve it, the result is Gayelord Hauser. Whenever large numbers of individuals are willing to make themselves ridiculous in the face of orthodox opinion, at a cost of which they are quickly made aware, there is likely to be an element among their motives that is not ridiculous at all. Much harm was caused by the liars and mental invalids who claimed to have seen flying saucers, but much harm was also caused by scientists who persisted in offering explanations that did not explain, insisting that no others were needed, and labeling all disagreement hysterical during the six years that elapsed before Donald Menzel's sympathetic, reflective, and apparently definitive book on flying saucers was published. If he is right, then the previous "explanations" were wrong; and they harmed science in their facile as-

sumption that all nonscientists are equally susceptible to hallucinations, and that all science was called upon to do was rap a few knuckles.

Hence a pronouncement like that of Michael Polanyi—"a society which wants to foster science must accept the authority of scientific opinion"—seems to me to be subject to considerable qualification. There can be no question of the right, nay, the obligation of scientists to decide for themselves what textbooks and journals will be published under their own auspices, what appointments will be made to their faculties and institutions of research, or to what projects their own time and effort will be devoted. Yet there seems to me to be a very large question whether this is the same thing as the acceptance of "the authority of scientific opinion" by nonscientists, or whether there is any substitute for free and open discussion on any questions that affect the entire society. The amount of money to be allocated to a National Science Foundation is just such a question, and the unhappy incident that occurred the first time it came up is highly illustrative. Among certain disrespectful nonscientists of my acquaintance, there was unseemly but understandable mirth when scientists, as a pressure group, lined up at the public trough with other pressure groups and suddenly discovered that they exerted no pressure. It was a salutary lesson.

One cannot be effective in politics while remaining above it. One cannot wield political power without accepting political responsibility, which is primarily the responsibility to respect the politics of others. I am very much afraid that ever since the threat of atomic warfare brought American scientists into politics on a large scale—and into government employ on an even larger scale—there has been a marked tendency among them to patronize the political scene, to sneer at it, and at the same time to seek to dominate it as a privileged caste. Like the poet, the politician must develop a healthy respect for facts, which are no less real because they are imponderable. He must manage a number of variables at least as large as the number of his constituents, and if his intuitive statistical processes for so doing do not average out successful answers, he ceases to be a politician. Here, on the other hand, is a representative sample of a "scientific" verdict on politics, taken from a symposium on cytology published by a college press:

We have to see to it that somehow future statesmen, members of the judiciary, the clergy, and other leaders of the people, the molders of public opinion, have a more "scientific" outlook than most now have. But in the meantime we scientists have also somehow got to take a larger part in the formulation of public policy than we have so far been doing. Here we run into a real problem: how determine policies without being a politician? And how can a good scientist be a good politician? For the essence of the politician's art is to make people think as he wants them to. . . .

Anyone who wonders why science has come upon hard times politically need only read that paragraph.

It is loaded with emotional assumptions that put a nonscientist's teeth on edge, and that lead nonscientists who seek to defend science into black despair. I am reminded of a physicist with whom I once discussed the "problem" of Anti-science; he said that it seemed to him perfectly natural that people should resent the scientist's superiority. There is a word for this, gentlemen, and the word is arrogance. It has nothing to do with science proper, it is not required by the needs of dedicated and impartial investigation; and it is certainly not sustainable on an evidential basis. It is an archaic prop to the ego, a social and psychological bad habit left over from the bad manners of nineteenth-century academic life, and fortunately it is already on the way out. But it is still one of the first and most unnerving aspects of science that many laymen encounter, and it has done incalculable harm.

V

A scientist might conclude, presented with these arguments for modifying the rigid definitions that separate science from other forms of human activity, that an effort was being made by laymen to penetrate science and to take over its time-honored functions. The prospect that existing distinctions might be blurred suggests this fear to Dr. Polanyi: "It would not only become practically meaningless to describe anyone as a scientist, but even to refer to any statement as a scientific proposition. Science would become, in effect, extinct." In all respect, I cannot share the logic of this defensive orientation. The opposite danger, that science might lose the fertilizing and revivifying contributions which amateurs have always made to it, seems to me equally great if not greater. And for science to lose contact with society at large would be, of course, disastrous. Error we shall always have with us, within the sacred precincts as well as without, and a dreary record of historical failures underlines the fallacy of supposing that any one group may purify itself and live apart. Is it an abrogation of the scientist's independent judgment to rejoin the race of common folk on more workable terms of equality than now pertain?

The "mad scientist" who is so consistent a figure of modern folklore is not entirely the product of envy and ignorance. There is justice—poetic justice, if you like—in the popular view of the archetypical scientist as a warped and incomplete being, a man who has isolated one component of the universal experience and cultivated it to the exclusion of all others. Science itself, in a historical perspective, has achieved its triumphs as well as its tragedies by imposing an arbitrary but significant order on the undifferentiated flux of nature. There is a sense in which science consists legitimately of distortion, in which one can say that all great scientific discoveries appear initially to be contrary to common sense, and in which the Western civilization that science has profoundly shaped now dominates the world precisely because it is neurotic. But it seems highly unlikely

that this pattern of dissociation can survive the coming fifty years without serious damage to both science and society, and of the powerful corrective forces now coming into play none is more hopeful than the urge of scientists themselves toward synthesis, both of one special field of study with another and of one with all.

That is why, as a nonscientist who wishes to see science prosper, I am relatively undisturbed at the image of a world in which scientists would be indistinguishable from people, in which scientists would be men and women first and scientists second, and in which—perhaps, in ways that scientists today may find difficult to visualize—everyone else will be scien-

tists, too. The human condition is crowded with ambiguities, and all our acts have unintended consequences. The act itself of posing the scientific dilemma in these terms will suggest to the reader countless other terms in which it might also be posed, perhaps irritating him where it ought to soothe and offering consolation where it ought to kindle wrath. These are emotional objects of dispute, charged with old quarrels and haloed with the motivations we impute to one another. They are not, in that respect, "scientific," but I commend them to the attention of scientists, lest they be left indefinitely in other, and ultimately less sympathetic, hands.

Some Comments on Popular-Science Books

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CONTRARY to optimistic rumors that have been circulating ever since the end of World War II, scientists still have not deserted their ivory tower. But the place is a good deal better ventilated than it once was. Moreover, further renovations are in sight, a fact that may prove heartening to those who have spent years trying to bring American scientists and the rest of the American public closer together. I might add that they are still far apart, and progress along such lines comes none too soon.

One promising sign is the bumper crop of books prepared by scientists for nonscientists. Popularizing is a vice that cannot be indulged in privately. Sooner or later, your efforts will probably be published—and not long ago that would have meant some loss of social status in the scientific community. Of course, your colleagues wouldn't have said anything to your face. But among themselves they would have wondered why you were writing instead of doing research, and concluded that you were slipping.

This attitude has not disappeared entirely. Traces of it can still be detected, particularly in the upper, less efficiently aired, chambers of the ivory tower. Pure mathematicians, snug in their hyperspaces, are most reluctant to take time out for book-writing, or for any other concrete form of popular science. The reluctance coefficient becomes smaller as one passes through the spectrum of the specialties from theoretical physics and chemistry to biology and finally to the social sciences. But generally speaking, writing for the layman is becoming respectable, and it may actually bring the scientist as much prestige as his achievements in research.

This article will confine itself to a series of comments on popular-science books, most of which have been published during the past two years. The ma-

jority concentrate on research currently under way or offer up-to-date summaries of scientific thinking on specific subjects. Some books are devoted to various themes in the history of science, and others, the smallest proportion of all, deal with science itself—its methods and aims and values. These three categories may not be all-inclusive; certainly they overlap in many cases. But they may help to indicate those areas of science that are relatively well covered and those that have been neglected.

A MATTER OF STYLE

As far as books in the current-research category are concerned, one of the most encouraging developments is an unspectacular but steady increase in the use of the word "I." This statement will have to stand as a general impression until some Ph.D. candidate investigates it statistically. But it is based on considerable reading, and several publishers have commented to me about the significance of the trend. In using the first person the scientist has taken an all-important first step in freeing himself from what is undoubtedly the deadliest, most awkward style ever invented by anyone for any purpose—the nameless style found in technical publications.

If the scientist insists on subjecting his colleagues to this sort of writing, that is his business (although judging by recent criticisms, they don't particularly enjoy it either). But the weight of experience shows that good English is more helpful in communicating with other people. Although uninhibited use of the first person is no guarantee that a book will be well written, it is a valuable index to general readability. The odds are that it will be easier to read than one which, in the name of being "impersonal," falls back on the phrasing typical of the average scientific report.

A good example of what I have in mind is *Stars in the Making*, by Cecilia Payne-Gaposchkin, of the Harvard College Observatory. This book, an introduction to stellar evolution, is written in an informal style that is certain to be appreciated by students, as well as by laymen. Also, it is a completely honest book and does not gloss over areas of ignorance in an effort to give an impression of certainty where none exists. In considering available evidence, Dr. Payne-Gaposchkin writes: "Of these tangled threads it is our problem to weave a coherent web; and I must be frank—the web is full of holes at present, and if I were to attempt to foist it off as a well-woven fabric I should do it at the price not only of compromising with my own conscience, but also of incurring the derision of my astronomical colleagues."

Such humility is not to be found in Fred Hoyle's *The Nature of the Universe*. The British astronomer has been accused of presenting controversial ideas and theories—namely, his own and those of his close associates—as if they had already been accepted by the scientific world. This fault, which is generally attributed to science writers rather than scientists, has justifiably aroused the wrath of astronomers in this country and abroad. Even some of Hoyle's harshest critics, however, concede that he writes beautifully and hope he will publish other, more restrained, books. In fact, *The Nature of the Universe* would be required reading in any course designed to teach people how to explain things simply and vividly.

Two less widely read books also deserve special notice as first-rate examples of science popularizing. The first, *The Physical Basis of Mind*, published more than two years ago, consists of a series of general statements outlining some modern concepts about thinking, consciousness, speech, and other functions of the brain. The statements were prepared by seven specialists including the Nobel prize winners Charles Sherrington and E. D. Adrian. The final chapter, a discussion of the mind-matter problem, presents some of the implications of modern brain research as interpreted by three philosophers.

Doubt and Certainty in Science by J. Z. Young, of University College, London, is also concerned with studies of the human brain. It presents the elements of cybernetics, emphasizing some of the more obvious similarities between the workings of certain centers in the nervous system and the action of high-speed electronic computers. Although written primarily for the layman, it may also prove of value to biologists who have difficulty swallowing—and following—cybernetical theories as described by mathematicians, physicists, and engineers. They may be more receptive to this book since it is written by a highly respected worker in their own branch of science.

It should be noted that the chapters of Professor Young's book were originally broadcasts prepared under the auspices of the British Broadcasting Company. *The Nature of the Universe* and *The Physical Basis of Mind* are also by-products of BBC programs

that aroused widespread interest among listeners. Broadcasting can stimulate the development of a colloquial style and the publication of popular-science books, because it encourages investigators to speak simply and informally. Popular writing is rarely difficult for those who can explain themselves without floundering about in technical jargon—and who consider it important to reach the layman.

In this country, however, network officials are not very enthusiastic about talking scientists. They will not experiment with the radio science talk—the *ad lib* talk or conversation—even on "educational" programs, which usually are scheduled for off hours and are not expected to attract sponsors. So far as I know, their sole contribution to popular-science libraries was a work that appeared shortly after the war. At that time Warren Weaver, of the Rockefeller Foundation, arranged to have prominent scientists deliver talks during the intermissions of the New York Philharmonic Society concerts. The broadcasts were later included in an interesting and informative book called *The Scientists Speak*.

SOME TRENDS AND IMPROVEMENTS

No discussion of popular science would be complete without a special place for Rachel Carson's *The Sea Around Us*. Having headed best-seller lists for well over a year, it should be considered a phenomenon rather than a mere book. About three decades ago Paul de Kruif developed an effective method of science popularizing. He invented, or at least promoted most successfully, the breathless detective-story style—a style that was epitomized by his *Microbe Hunters* and can be credited with creating a far larger audience for books about science than would otherwise exist.

But a breathless style requires a breathless subject, and de Kruif's has unfortunately been applied to a host of minor topics (by the originator, as well as by his many imitators). We keep reading exciting lead paragraphs, anecdotes full of suspense, phrases like "the balding, sad-eyed researcher." As often as not, however, the "discovery" being described represents the quintessence of anticlimax. It may be a mere molehill of a discovery, far too insignificant for such a mountainous style. We are informed about a new balm for athlete's foot, or a wrinkle-removing hormone cream, or a portable dehydrating unit for family picnics. No style, however rugged, can stand up indefinitely under such maltreatment.

This may be the strategic time for a different approach, and *The Sea Around Us* may indicate the nature of that approach. It is essentially a literary way of writing about science. The objective is not only to inform, a process which may satisfy the reader but cannot in the long run satisfy the creative writer. Rachel Carson tells us many things about the sea—and she also offers a work of some artistic distinction. She demonstrates once more that the findings of scientific investigators can contribute to our aesthetic ap-

preciation of nature, as well as to our factual knowledge. In other words, she is doing a great deal to emphasize the humanistic and cultural aspects of science—far more, by the way, than the perfunctory and uninspiring sessions of the average “science appreciation” course. If this sort of approach develops into a trend, we can expect a new type of book and, probably, a new type of author.

Another welcome improvement concerns the material used to illustrate books for laymen. Nowadays one of two things can be expected. The publisher and author meet in joint conclave shortly after the manuscript has been completed. They either decide that no illustrations are needed or—and this is the more common course—agree that it might be a good idea to have something besides straight copy. From that point it is usually a matter of rounding up photographs and diagrams previously prepared for other purposes. If any special illustrations are required, an artist can be called in to complete them (at as low a price as possible). When the book is published, the illustrations may be attractive and even somewhat helpful, but are probably not integrated with the text. Basically they are afterthoughts. They may be simpler or more complicated than the text, more formal or less formal. If they fit, it is usually sheer coincidence.

Ideally, the artist would not be summoned after the text had been completed. He would come in at the very beginning, have a chance to read chapters in the rough, and possibly make suggestions for rewriting so that text and illustrations would supplement each other more effectively. Such collaboration between artist and writer has long been the rule in preparing special science stories in *Life* and other picture magazines. It was also practiced extensively during the war to prepare booklets describing the construction and use of new weapons that involved electronic circuits, acoustic mechanisms, and other unfamiliar gadgetry. The booklets had to be written and illustrated as clearly as possible, since they were to be read by laymen of all ranks, from able-bodied seamen to admirals. One of the expert artists who worked on these projects, Sol Ehrlich, is now responsible for the extremely attractive layouts of *Physics Today*.

Most publishers, if cornered, will concede that illustrations could be improved—but not within the limits of present-day budgets. On the other hand, there seem to be ways around the problem that might be studied and applied more often. Two books on space travel, *The Conquest of Space* and *Across the Space Frontier*, include admirable illustrations by Chesley Bonestell and other artists. Interestingly enough, these pictures are well ahead of the texts they go with, as far as the popular touch is concerned (this is a compliment to the illustrations, not an insult to the texts). For almost perfect meshing of text and illustration, interested readers are referred to *The Stars—A New Way to See Them*. The book is unusual in that its author, H. A. Rey, is not only an able popularizer but an excellent illustrator.

Finally, I look forward to the day when publishers will discover a large and, according to my informants, rapidly developing area of science—biology or, rather, biological research. Many good books have been written on various aspects of descriptive or naturalistic biology, books that explore the habits and customs of such creatures as spiders, beavers, octopuses, and bees. Medicine is also well represented. A wide selection of titles is available to people who would like to go into the care and treatment of heart trouble or stomach ulcers, or who would rather read about than undergo psychoanalysis. But most people still believe that physicians were responsible for the development of penicillin, plasma expanders, and other advances for which biologists deserve the major share of the credit.

A series of books that would do justice to basic research in biology might require careful planning, but the main burden need not and should not fall on the publisher. There are qualified investigators who would be glad to serve as consultants and writers, and artists to do the illustrations, although it would not be fair to ask for their assistance without more generous compensation than is customary at present. It is possible that a private foundation might be willing to help support the undertaking. With certain interesting exceptions, the foundations are more sympathetic toward such projects than they were only a few years ago. If the series were successful, it would be as instructive and stimulating to the layman as Ernest Baldwin's *Dynamic Aspects of Biochemistry* is to the biology student.

THE HISTORY OF SCIENCE

Most of the books discussed up to this point focus sharply on current experiments and theories. We shine at publicizing the present. Apparently a thing has to be new to be interesting or, if it isn't new, it must at least be used for a new purpose—e.g., chlorophyll. The American public is probably better informed about the progress of research than any other public in the world. But the history of science has received little notice and is widely misunderstood. Too many people think of the past as a series of sudden flashes of discovery, a long line of brilliant individuals shouting “Eureka!”

If I seem too pessimistic, there are good reasons. Among misguided intellectuals, we find men like Arthur Koestler who, in *Insight and Outlook*, builds a strange philosophy, and a naïve one, on the Eureka myth. The notion that discovery bursts upon one has been thoroughly and repeatedly discredited, although it is still a favorite device of hack science and radio script writers. There is ignorance at the other end of the intellectual scale, too.

While I was writing this article and had already run a day or so over my deadline, I turned on the radio and found myself listening to a quiz program. The following problem was under consideration: “Tony Pastor and Louis Pasteur are both known as wonderheads in their own fields. Which was the

French chemist?" The man being quizzed had no trouble at all in providing the master of ceremonies with the correct answer. Although he had never heard of Pasteur, he had half the total information, which was sufficient. Unlike most readers of SCIENCE (I assume), he knew who Tony Pastor was and figured things out by a process of elimination. The incident surprised me, mainly because Hollywood had done a motion picture on the life of Pasteur (although the film has not been revived recently).

This experience may not be typical, and it is certainly not cited as an accurate indication of the scope of the problem. Things are probably not that bad. There are many valuable and penetrating studies in the history of science which can be readily followed by laymen. To cite one of the latest examples, Giorgio Abetti's *The History of Astronomy* has recently been translated. It contains some good anecdotes, a record of technical advances, and interesting biographical sketches of leading personalities from Tycho Brahe to George Ellery Hale. This book is part of the "Life of Science Library," which includes more than twenty other books dealing with great men and great ideas in scientific history. Henry Schuman, the publisher of this important series, is to be congratulated for his active role in making such material available to the reading public.

But can the history of science be brought to even larger audiences—audiences as large as those that follow current aspects of research? There is every reason to believe it can, and books might exert appreciable influence in a well-organized effort. (Radio and television shows and motion pictures, of course, would be more effective in conveying attitudes and feelings on a mass basis.) The task of reaching more people, however, might call for some changes in emphasis. Certain educators believe that the way to teach science as a cultural subject—that is, to students who do not wish to become scientists—is to concentrate on the achievements and ideas of the past. For example, the period of Priestley, Lavoisier, and the phlogiston theory would be analyzed in detail to illustrate basic principles used in scientific investigation.

Such an approach may or may not prove itself satisfactory for students of college age, but it is usually ineffective in popular books or any other form of adult education. As a general rule, the cards are stacked against the scientist or science writer who confines himself to the past and at the same time hopes to be read widely. No matter how vividly and cleverly he writes, he may have trouble combating the feeling that, after all, the events took place long ago and involved people no longer alive. Often an indirect approach may be more suitable for introducing historical subjects.

In planning books that are intended to go into significant developments of the past, it might help to experiment further with the idea of focusing sharply on the present. The idea would be to select a currently active field of research—say, the design of

electronic computers, or virus investigations—and describe some of the most interesting current work, using the best techniques of popular-science writing. Then turn back to earlier times and show how modern developments are simply the latest results in a long sequence of observations and theories. This roundabout way of presenting history is not new. In fact, it has been used in practically every good account of atomic research. The latest of these books, *The Atom Story*, by the British biochemist J. G. Feinberg, starts with ancient Greek science and proceeds through phlogiston theory and many other topics before coming to grips with the hydrogen bomb.

Whatever the methods employed, the history of science needs better publicizing. As things stand now, science is almost always presented as the creator of new gadgets and notions, a kind of brash young interloper in the world of culture. That the "interloper" has actually been around for a long time, and boasts an ancient and respectable ancestry, might be emphasized from time to time, if only to reassure those who do not yet understand what science is up to. As George Sarton expresses it, the history of science should be used "to illustrate impartially the working of reason against unreason, the gradual unfolding of truth, in all its forms, whether pleasant or unpleasant, useful or useless, welcome or unwelcome."

THE VALUES OF SCIENCE

The rarest thing of all is a really popular book about science itself, a book the primary purpose of which is to present for all to read the values expressed by Dr. Sarton in the above quotation. Popular science has not yet succeeded in what should certainly be one of its most important missions. There is one particularly spectacular sign of this failure, the wide sale of books that present fantasy as if it were fact—and get away with it. Someday we may have a public sufficiently sophisticated to resist such claptrap, and to resist despite the occasional high-pressure methods of publishers who should know better (and probably do). The millennium cannot be expected, however, until people have learned to distinguish science from science fiction, and that means a serious and vigorous campaign of public education.

An important step in such a campaign would be to expose fakery as soon as it appears and in no uncertain terms. Scientists have long been doing this on an individual basis, in scattered articles and book reviews. The most recent and most effective blast against pseudoscience is *In the Name of Science*, by Martin Gardner, a free-lance science writer who was formerly on the public-relations staff of the University of Chicago. The book identifies and describes a long list of pseudoscientists, past and present. There are flatworlders and antievolutionists and people who cure diseases with colored lights. Dianetics, that unholy alliance of psychoanalysis and cybernetics, rates a special chapter. So do dowsing rods, food fads, and flying saucers. One of the most important things about

the study is that it indicates how vast the field of pseudoscience actually is. It includes work that is not entirely nonsense and that may even have acquired a measure of respectability.

In the Name of Science deserves the large audience it will probably have, particularly among scientists. For one thing, the antics and arguments of pseudoscientists make first-rate reading. Even more important, this new book is something that should have been prepared, or at least sponsored, by the AAAS or some other scientific organization interested in public education. I am aware of the argument that an official objection to various works of pseudoscience might only draw attention to them and thus boost sales. The only catch to this argument, however, is that—judging by a brief survey of recent best-seller lists—such dubious volumes do quite well on their own. Silence does not seem to have affected sales records.

Also, silence cannot be expected to discourage either the writers or the publishers of pseudoscience. The assumption here is that in the long run such material harms science, that it fosters misunderstanding of the nature and aims of research. If there is anything at all to the assumption, pseudoscience certainly ought to be opposed actively and aggressively. Or is the American Medical Association making a mistake every time it attacks a cancer quack? It happens that, in exposing the efforts of fakers, one must inevitably give some idea of genuine research. The layman will learn a good deal about scientific evidence and standards from *In the Name of Science* and also from

Flying Saucers by the astrophysicist Donald Menzel.

Much of what has been written about the humanistic values of science appears in books devoted to other subjects. For example *Life of the Past*, an introduction to paleontology by George Gaylord Simpson, includes such comments as the following: "There was no anticipation of man's coming. He responds to no plan and fulfills no supernal purpose. He stands alone in the universe, a unique product of a long, unconscious, impersonal, material process, with unique understanding and potentialities. These he owes to no one but himself, and it is to himself that he is responsible. He is not the creature of uncontrollable and undeterminable forces, but his own master."

I would recommend sections of this and other books to a large and increasing number of laymen, who are not basically interested in paleontology or the history or current achievements of science. They want to know what it is doing to their ideals, their notions about truth and purpose. They want to know what science has to say about the values of the past, and whether it has any values of its own to offer. Not enjoying the benefits of a technical education, they are supremely unimpressed with the fine points of the argument that science is not concerned with matter of right and wrong. They find it difficult to make the distinction between the scientist as scientist and the scientist as human being. Popular books have not yet been written for this audience—that is, they have not been written by scientists. So far, we have heard only from the pseudoscientists.

Scientists *Can* Talk to the Layman

John W. Hill and James E. Payne

Hill and Knowlton, Inc., and American Iron and Steel Institute, New York

COMMUNICATION has become an irksome, two-pronged problem for the scientist. On the one hand, he finds it increasingly difficult to keep abreast of the work in his own and allied fields; on the other, he sees an ever-widening gulf separating him from the public.

Expanding research programs yield data at an accelerating rate, yet the scientist's reading and retention rates are limited by physiological and psychological factors. "If I kept up with all the work being done in the narrow field of antibiotics alone," a chemist observed recently, "I would have no time left for research. As it is, I am buried under a mountain of papers and reports."

Fortunately, scientists realize the seriousness of this bottleneck, and undoubtedly it will be removed before it strangles scientific work. Scientific language, with its mathematical symbology, is universal; conse-

quently, the problem is largely one of engineering. Once a method is set up by which information can be abstracted at various levels of complexity, recorded, cross-indexed in efficient research pathways, and made available in easily accessible form, the scientist will no longer need to flounder through unnecessary data to find what he needs. Increased reading efficiency will enable him to keep informed of developments.

But the problem of communication between the scientist and the public has no such obvious solution. The scientist is changing the world about us. His work is vital to our health, security, and prosperity. Yet to the average layman the work and the language of science are as mysterious as the witch doctor's mumbo jumbo is to the savage. The pace of scientific discovery has left the layman far behind, and the few interpreters of science too frequently speak a language he does not understand.

The urgency of this need to improve communication between the scientist and the public should not be underestimated. Ignorance breeds suspicion. Understanding and a cooperative spirit go hand in hand. *Science needs the cooperation of the public and the government today. The need could become desperate tomorrow.*

Industry has a similar problem in communication. The sales department of a large chemical or electrical corporation has the monumental task of educating the public to accept and use an ever-growing list of scientific wonders translated into products designed to improve the standard of living. The public relations departments of the same corporations have the double task of translating the basic scientific and engineering facts about the products into terms the layman can understand, and at the same time helping him to realize the scope and importance of the research programs that made the products possible.

In the past few years industry has made progress toward solving this problem, particularly in the development of more effective controlled-circulation magazines. It has developed effective techniques for translating technical developments into nontechnical terms—techniques that may be of value to scientists interested in the basic problem of communication.

Industrial magazines with a controlled circulation fall roughly into two categories: internal and external. The internal publication is directed primarily toward the employees of the company sponsor, and ideally is designed to relate the employee to his job, the job to the company, the company to the industry, and the industry to the economy as a whole. A well-written, well-edited internal magazine can be a potent tool for molding good employee relations.

The external can have one of two primary goals: to carry the message of the sponsor's sales department to customers or potential customers, or to speak for the sponsor to a selected list of opinion makers as a part of a public relations program. Both internal and external magazines are wholly subsidized and distributed free of charge to a selected list of readers.

Publications in both categories vary widely in conception and treatment. Annual budgets range from a few thousand to several hundred thousand dollars, and the literary and artistic qualities run the gamut from amateurish to highly professional. Some industrial magazines now rival the best of the "slicks" in every respect. Most of them are actively attacking the problem of breaking down the barrier between the scientist or engineer and the layman.

The magazine *Steelways*, which is published by Hill and Knowlton, Inc., as a public relations service for the American Iron and Steel Institute, is atypical in some ways, since its purpose lies entirely in the field of public relations, and it is published for an industry rather than a single company. But insofar as the basic problem of translating technical material for a lay audience is concerned, its job is identical with that of most other industrial magazines. It may be of benefit

to scientists and scientific organizations, then, to discuss some of the lessons we have learned.

One of the most encouraging things is that the layman has an enormous curiosity about science and technology—and by "layman" we mean not only men in every walk of life, but women and young students as well. In the field of science the layman is like a child: he wants to know what is going on behind the closed door. To carry the analogy further, he is a shy child; he does not want to admit his own ignorance. His apparent lack of interest, his indifference to technical information, is a well-learned defense against that too often closed door.

Comments like these: "I've wanted to know that for years, but I thought such things were beyond me;" or "For the first time I understand what my husband's job is all about!" are common reactions to *Steelways* articles. Often the letters are from readers who could not or would not read scientific material.

But simply opening the door is not enough. If you take that same reader into a chemical laboratory and lead him carefully and patiently past every item in the maze of equipment, he will not only tire quickly; he will learn little or nothing. Key facts must be abstracted from the confusing mass of information, described in terms he can understand, and related to his own everyday world to give him a meaningful picture of the purpose and work of a laboratory.

Here we encounter our first major problem. Much of the scientist's work lies outside the world of direct sensory experience. We cannot see or smell or taste or feel an atom. An electromagnetic wave is an abstraction of an abstraction. The only language that can speak literally here is mathematics, and to the layman mathematics is as meaningless as Tibetan or Hindustani.

Here, a picture or a drawing or an image is worth a thousand words. Although we cannot see an atom, we can draw a picture of its behavior. It is simple to illustrate the precession of a gyroscope in diagrams; it is well-nigh impossible in words. Where words fail, too, we can fall back on descriptive ideas, or images. By taking poetic license it is often possible to describe a difficult concept "as if" it were something simpler, but of the same order of function. It is not always necessary to be exact.

The atomic physicist, for example, does not "see" an atom as a literal clump of ball-like protons and neutrons encircled by electrons resembling infinitesimal BB's. Nor does he see the atom as a sun surrounded by planets. But both these pictures enable the lay reader to feel an understanding of the physicist's work. The more abstract the idea or concept, the more necessary it is to find some image or symbol that will permit this "feel" of understanding.

There is still another approach that is surprisingly effective. Granted the need to impress the reader with the importance of a project or process that is too complex or too far removed from experience for him to understand, we can achieve a degree of communi-

education by tying the project or process to the men who developed it, or to the men who use it. The interest in the men will often encompass their works. "Relativity" is a household word today because writers have made Einstein a friend of the family. The result of this technique is nearer sympathy than understanding, but it is communication, and it creates an effective bridge between scientist and layman.

Finally, we have learned how imperative it is to associate the idea or concept with everyday experience and needs. If we can show the connection between the idea and the reader's health, security, or prosperity, the problem of communication becomes immensely simplified. Within limits, we learn what we need to learn; we are sympathetic toward those things that experience has taught us have survival value.

When an article is written with these lessons in mind, even though it deals with extremely difficult technical material, the effectiveness of the communication job is startling. From an average issue of *Steelways*, articles are reproduced in whole or in abstract by about 40 magazines per month; news stories based on the articles appear in newspapers with a combined circulation of ten million; scripts based on *Steelways* stories reach an average television and radio audience of over ten million, not to mention periodicals that do not credit the source.

Recently we sent a postcard query to each of the 11,000 teachers on our mailing list, asking them if they used *Steelways* in their classes, and if so to what extent. We expected a 15 per cent reply; instead, 45 per cent replied. Of those who answered, 5006 stated that they use *Steelways* in their classes regularly, against 98 who do not. The 5006 teachers use the magazine in 14,707 classes, serving 244,327 students. Yet over 75 per cent of the material used in *Steelways* is technical or semitechnical in nature!

Results such as these leave little doubt as to the interest of the audience, or the feasibility of reaching it. The potential is there if we are willing to devote the time and thought necessary to realize it.

The scientist can play a key role, although there are not many men like the late Sir James Jeans. The scientist who is also a writer is a phenomenon that providence grants us all too infrequently. But even the nonliterary scientist can help a great deal if he will recognize the importance of the writer's job and cooperate with him.

Steelways writers have had some interesting experiences working with scientists and engineers. Although both groups breed a certain percentage of members who have a built-in intellectual sneer for anyone outside their own field, fortunately such individuals are in the minority. If the writer is sincere, and if the magazine gives the expert the right to correct the finished copy, the average scientist will give generously of his time and his knowledge.

The difficulty here is to persuade the scientist to come down from his intellectual heights and see the writer's task at eye level. The work is familiar every-

day routine to the scientist; it is likely to be a dark mystery to the writer. He needs first of all to know what the project or process looks like, feels like, smells like, sounds like, and perhaps tastes like.

If it is classified (what a wonderfully ambiguous word!) perhaps he can get a secondhand description that will have some meaning for the reader. If the subject is by its nature beyond sensory experience, like an atom, he can see charts, diagrams, cloud chamber pictures of atoms being smashed. Anything that the scientist can give him in the form of models, photographs, or visual aids of any kind will help him lead the reader into the story.

Now we come to the tough part of the assignment. The writer knows that his most powerful tool is "human interest" material. If he can tell the reader something about the man who developed the process, or the men who are working on the project, half his job will be done. But scientists are a shy lot, and they are as jealous of one another as a bunch of club-women. So it is a bold scientist, indeed, who will give a writer those personal details of which human interest material is built. But if both scientist and writer are wise, it can be done with dignity and safety.

Tangent to this problem is the writer's never-ending search for "incidents." The scientist is understandably cagey about "incidents," since they usually are humorous at his expense, or record moments of danger or disappointment that he would prefer to forget. Yet incidents are the hooks the writer uses to grab the reader's coattails and make him listen to his story. No matter how great his dignity, the scientist should remember that an entertaining human sidelight on a difficult technical subject often means the difference between being read or not read.

The scientist should remember, too, that when he is talking to a writer he is no longer just a chemist or a physicist: he is a translator as well. It is his job to take the language of science and break it down into terms the writer can understand. If he fails, the writer is helpless. At best, the article will be obscure. It may be erroneous and damaging both to the magazine and to the scientist and his work.

Finally, when he has cooperated with a writer in giving information, the scientist not only should be willing to review the finished manuscript; he should insist on it. In most cases the writer is stretching his knowledge and understanding to the utmost. It is to be expected that his effort will contain errors. It should be part of the scientist's job to correct them—in the writer's nontechnical language.

The importance of this communication job cannot be overestimated. Science and industry, working together, have given us better tools for communication than any man would have dreamed possible fifty years ago. If we use these tools effectively, we can span the gulf between the public and the scientist, and between the public and industry. If we do not, we may find ourselves with a gulf so wide it cannot be bridged.

Selling Books to Scientists

Martin Matheson

John Wiley & Sons, Inc., New York

MANY PEOPLE can write books; it's no great trick to manufacture an attractive book; but it takes a lot of doing to sell a book." So I was told many years ago, a few days after entering the employ of Wiley. A member of the staff, a well-known bookman, was repeating to me an observation that he himself had heard over and over again during his half century in the book business.

At the moment the remark seemed relatively unimportant. But, as I grappled with the problems of my new job, I came to understand the hard facts behind the old saw. I gradually got the idea that the unknown (at least to me) individual who had said it first had uttered a truism to which publishers will always give complete assent. In fact, it is safe to say that the observation has become almost a cliché.

I had come into publishing by way of a large industrial company, where I had been exposed to production, sales promotion, advertising, and selling. I had been appalled at the great waste involved in trying to solve advertising and sales problems. This experience had been invaluable, and in my innocence I thought that for my new job the greatest lesson it had taught was what not to do. Some waste was inherent in any distribution system. No one deliberately wasted promotion money. To reach your prospective customers you spent x number of dollars to reach y number of buyers; and, as you did it, you were well aware that perhaps only three quarters of the circulation could be by any stretch of the imagination be considered possible purchasers of your product. Well, when I got into specialized book publishing, where every published book had a specific audience, I had the naïve notion that at last here was a business where waste and inefficiency could be reduced to a minimum.

But it just wasn't that simple. I was still confronted with unchanging human nature, habits, social and scientific mores—in fact, all those intangible factors that are so vital a part of modern sales promotion.

Before proceeding to a specific discussion of the subject assigned to me, I should like to make one or two preliminary observations. It is important, for instance, to assume from the start that the book is a good one, that it has had competent editorial direction, and that a market survey of the field has indicated the need for a book of this type, scope, and size. I should also like to suggest that the publisher has other motives in publishing books than to sell them. Without adequate sales, he could not, of course, stay long in this fascinating business. But the publisher worthy of his calling wants above all else to issue

books that will make a definite contribution to the literature. He also derives pleasure and stimulation from contacts with his authors, whom he has come to regard as his partners in a worthy enterprise.

I am reminded of a dream recently recounted to me by a publisher. It was a marvelous dream and it gave him considerable satisfaction that he was able to remember it in detail. He had just published an exceptionally fine book on a subject of increasing importance. He had taken a full page of advertising in the only publication reaching this group, and a circular had been sent to a selected list of prospects. He had heard that the reviewer for *SCIENCE* was one of the leading authorities in this area of knowledge and was sympathetic to the author's views, even those on controversial issues. Up to now there was nothing unusual about all this, but here is where the unrealities of the dream world come into the picture. What a sweet dream it was! He dreamed that on the day the issue of the specialized journal appeared, every subscriber dropped whatever he was doing, took the wrapping from the magazine, and sat back in an easy chair to give complete attention to its pages. And lo, every man jack stopped when he came to the publisher's announcement of the new book and read every single word of copy. Not only that, every interested reader took immediate action: He did not wait; he wrote that minute for an on-approval copy.

The wonderful dream goes on. Our publisher saw vividly a really tremendous miracle happen to the eight thousand circulars his mailing division had sent out. Though the mailing piece was a good one, it actually was not exceptional. Yet this time the incredible, the impossible, was taking place. Every envelope was opened, despite the fact that the recipient knew at once that he was about to read a book advertisement; and, *mirabile dictu*, the reader, instead of throwing it all unopened into the nearest wastepaper basket, avidly read both circular and letter; and those for whom the book was written immediately filled out their order forms and rushed with them to the nearest mailbox.

And now for the final act of this most excellent of dreams. *SCIENCE* came out with a column-and-a-half review of the book by an enthusiastic critic. He hailed it as "a brilliant piece of expository writing, a classic of its kind," and, most important of all, "an invaluable tool and indispensable source of inspiration to all workers in the field." Again lightning struck. Every reader of *SCIENCE* turned automatically to the review, read it with enthusiasm, and "all the workers in the field" promptly ordered the book. Of course the

first printing was quickly sold out. Orders for a substantial second printing were about to be issued when our dreamer awoke from his beautiful fantasy.

From our point of view, the publisher's dream has many omissions and comes to an end all too quickly. But why not continue the dream for him? We can envision, for example, a situation where the hundreds of libraries and educational institutions throughout the world take immediate cognizance of our new book, and all the leading bookstores display it prominently. In our dream it would seem appropriate for all foreign-language journals in the field to publish good reviews within six months after publication. Any good bookman could go on in this fashion ad infinitum, but I am sure enough has been said to suggest some of the marketing problems of importance to the technical and scientific publisher.

Why is this dream so distorted a picture of what actually takes place? The reader of this article can supply his own answer. He knows precisely what in his own experience deters him from doing what every magazine publisher and advertiser wants him to do. Someday an unbiased research agency will make a study of a subscriber's reading habits with regard to advertising. Of course, continuous studies are being made on various aspects of the problem. Publishers of mass media hire experts to appraise the readership appeal of their editorial content, and advertising agencies are constantly trying to measure quantitatively and qualitatively the effects of various copy techniques. But no one, so far as I know, has asked the pertinent questions that will tell us even approximately how much profit an advertiser gets for his advertising dollar.

Book publishers, too, at one time or another conduct research to get answers to promotion problems. Many of them are routine checks to determine the selling power of specialized journals in which they advertise, and the efficiency of mailing lists, etc. But we know also that publishers, even those who have been in the business a long time, are trying to find the answers to still more fundamental questions. What sells a book? Who buys books, and how many? Where are they bought? If these questions seem naïve, even ridiculous, remember that research has never provided answers we can regard as definitive. We make studies and we think we have the answers. We try out the new formula, which seems to work for a time; then, six months later, we discover that it does not work at all. New studies reveal a contradictory answer, and we start all over again. But research is by no means fruitless. If nothing else, results of such studies reveal the hard and humbling fact that there are no absolute answers.

Big advertising agencies have always insisted that they have the correct answer to book promotion. They smile at our small budgets and actually believe they could make book publishing as big and profitable as any other type of business—cosmetics, cigarettes, etc. From time to time trade publishers have listened to

this siren song and have risked the thousands of dollars necessary to advertise in large-circulation media. But we notice that the gamble is not repeated, and that their advertising subsequently follows accepted book advertising standards. The old hackneyed retort, "Ours is a different kind of business," certainly applies to book publishing. If we manufactured clothespins, there would be no question as to our profound faith in continuous promotion of the product. But the book publisher has not one product, one book. He has a hundred or more products, each requiring a special kind of promotion if it is to be sold successfully. He has to use all the known techniques that apply to the distribution of other products: journal advertising, direct-mail, book displays, publicity, sampling, and personal selling. How he uses them, and in what proportions, largely determine his efficiency as a publisher.

Here is as good a place as any to state categorically that advertising can point out the merits of a book, but it cannot add a single virtue that is not already there. For example, to say that a book of only 116 pages is "without question the most comprehensive treatment of the subject now in print," is likely to be an unwise overstatement. Perhaps the author intended the book to be merely a summary of important research going on in the field—significant, yes, but not "comprehensive." Naturally, the publisher is enthusiastic about his book because he has backed his judgment by a substantial investment. But he is also well aware of the necessity of adhering strictly to the truth, as he sees it, in his advertising. Moreover, he knows that there is no advertising in the world that can give to a book a quality or a superiority it does not possess.

In a journal such as *SCIENCE*, it is not out of order, it seems to me, to make some comments on the part played by the review in selling books. Two years ago Wiley ran a study on the effectiveness of various selling methods. The results showed that critical reviews ranked immediately behind direct-mail advertising. I use the word "critical" advisedly in describing a book review. Too many reviews consist of a summary of the book's contents; others repeat the publisher's jacket blurb or his circular copy. The review that really is significant and useful to potential readers is the one that can be written only by an authority in the field who has given the book careful study. He must even read the preface to make certain he understands what the author has tried to do, and above all what he has intentionally omitted. The signed review, the practice of most of the better scientific journals, is usually a much better performance than the unsigned one. If a book reviewer knows that his comments will appear over his name, he will in the nature of things exercise more care and will be more thorough in evaluating the book. In the final analysis, he is working for the prospective user, and stating as frankly as he can whether a book should be bought or avoided.

In making textbooks known to teachers, the complimentary copy distribution system is, despite its obvious limitations, an effective selling technique. Publishers' representatives and the office staff use the utmost care in placing on the "comp list" only those teachers who are logical potential buyers of the book for classroom use. Nevertheless, we know from our talks with other publishers, and from our own experience, that copies are too often given away to those who cannot possibly adopt the book. Teachers commonly assure us when we submit a sample copy for inspection that they require no follow-up. Teachers, however, are subject to the same human frailties as publishers and need to be reminded by personal calls, letters, advertising, etc., that the publisher is interested in their opinions and decisions. Perhaps we shouldn't mention it but we have known professors who have not even taken the book from its wrapper, and on a subsequent call our disappointed traveler has found it on a convenient catchall table in the harassed professor's office, along with his unread journals and catalogues.

In a recent *Wiley Bulletin* editorial, our copy editor made some comments on this matter that I think deserve repeating:

Complimentary copy distribution is undertaken by textbook publishers, as most readers know, with two major purposes in mind. First, we hope that a textbook will be adopted, after full consideration, in enough educational institutions throughout the world to justify the time, effort, and money invested in it. Second, and just as important, we should like as many opinions as to the quality and usefulness of our books as can be obtained for our future guidance.

We look upon the opinions we receive on our books as an important part of our continuing survey of textbook needs. Travelers, editors, consultants, and reviewers quite naturally give us much helpful information about our books. But also of great importance to us are the comments from teachers who are vitally interested in the publication of good textbooks.

No discussion of book promotion is complete without a passing reference to a publisher's traveling sales staff. It has been said more than once that the representative is the company's eyes, ears, and voice. In his visits to colleges, libraries, bookstores, laboratories, and industrial plants, he is indeed the company, interpreting its policies and selling its books. He is more than that, however. As he makes his rounds he becomes aware of new trends in teaching and in education, which he reports faithfully to headquarters. His effectiveness depends on more than a winning personality. The good traveler has a specific knowledge of his own and his competitors' books, and can provide assistance in solving teaching problems. He must know which books are suited to the instructor's needs and which will not apply to them; frequently he can provide information on the experience of other teachers in similar situations. As he develops experience and judgment, he becomes a valuable link between

potential authors and the company's editors. The competent traveler, and there are more than a few, becomes a friend and counselor to many. As such, he is appreciated as much by his clientele as he is by the home office.

Throughout this paper I have referred to a number of techniques used by the specialized publisher in promoting his books. There are other ways to sell books, and it must be stressed that today's publisher is not satisfied to depend on, say, the use only of circulars and the services of his traveling sales staff. He must use all the available methods, or he may be overlooking a technique that might be the one superior method for promoting that particular book. In selling his product, the publisher is judged by how well he performs the tasks involved in the intelligent use of the known selling methods. For example, in writing advertising copy for circulars, it is fairly easy to write according to a pattern, making use of the clichés of high-pressure salesmanship. But it is much more difficult to prepare copy that tells accurately, simply, and succinctly the important features of a new book. Superlatives are not needed to impress the scientist. He wants primarily the salient information about the book and its author. He does respond to informative and arresting headlines, and to sound and interesting writing and attractive layout. In advertising he appreciates the allover good taste that is an integral part of the important job of publishing high-quality books in the several fields of modern science.

Occasionally a book does not sell because it is ahead of its time. No amount of promotion, no amount of critical praise, will help it much until the field has caught up with it. A book like Getman's *Theoretical Chemistry* is a good case in point. When it was published in 1913, few colleges offered formal, systematic courses in the subject. Gradually, with a good book available, courses were established, and soon after World War I it was used in practically all the major colleges in the country and in many foreign universities. Today Getman's pioneer text has now become Farrington Daniels' *Outlines of Physical Chemistry*, undoubtedly still the largest-selling textbook in the field. Another title that started off with many of the characteristics of a failure was Coffin's *Vector Analysis*. In 1909 mathematicians were little interested in this subject, but as time went on the book sold in increasing numbers. An interesting fact is that it had its best sale in 1947, thirty-eight years after publication. In such instances one cannot say that these books were not good ones from the very beginning. They were, but for their complete acceptance both authors and publishers had to rest their souls in patience until the time was ripe.

After we have mentioned most of the known ways of selling a book, the fact remains that the most effective, and to authors and publishers the most cherished, of all the various kinds of advertising is the one that cannot be bought—word-of-mouth advertising. This, of course, is not a technique at all. It is a result

brought about by a happy combination of circumstances where many people spontaneously and voluntarily talk enthusiastically about a book. The publisher cannot arrange such things. They just happen.

In prewar days it was fairly common practice to use the so-called saturation method of book promotion. This can be likened to the use of a shotgun in covering broadly every possible segment of a given area. With current high costs, it is becoming more and more necessary to use the rifle. Today, the publisher must be more selective in the use of media. He cannot afford to try to reach every single possible purchaser of a given book. It simply is not sensible

to spend one hundred dollars to get back five. Fortunately, there are other methods, direct and indirect, as previously mentioned, to reach the fringe prospects.

If you have concluded from the foregoing comments that publishing scientific books is a difficult, unpredictable, and hazardous venture, you will be right. It is a satisfying business, however, with its own special rewards. The publisher does his best to put out good books, books which experience and research tell him can be sold with mutual benefit to an intelligent and highly specialized audience. But that audience must be reached, and the publisher is ever reminded that "it takes a lot of doing to sell a book."

News and Notes

Scientists in the News

President Eisenhower has accepted the resignation of **Allen V. Astin**, Director of the National Bureau of Standards. The resignation was requested by the Secretary of Commerce, **Sinclair Weeks**, one reason given being the bureau's adverse report on a substance designed to prolong the life of electric storage batteries. **Secretary Weeks** has stated that the bureau's tests on the products are not "sufficiently objective because they discount entirely the play of the marketplace." Dr. Astin has stated, however, that the bureau made several exhaustive tests of the product and found it to be of no value. The Bureau of Standards has a Board of Visitors established by law to advise the secretary regarding the operations of the bureau. This committee, which was not consulted in the matter by **Secretary Weeks**, consists of **Robert F. Mehl**, Carnegie Institute of Technology, chairman; **Detlev W. Bronk**, president, Johns Hopkins University; **M. J. Kelly**, president, Bell Telephone Laboratories; **Donald H. Menzel**, Harvard Observatory; and **J. H. Van Vleck**, Harvard University. A new committee is being set up by **Secretary Weeks** "to evaluate the present functions and operations of the Bureau of Standards in relation to the present national needs." The situation thus created is one of great concern to American science. All facts on both sides should be presented to the public in the interest of both scientific integrity and national welfare.

W. I. B. Beveridge, head of the Department of Veterinary Pathology, University of Cambridge, has been appointed visiting professor of bacteriology for the spring quarter at the Ohio State University.

Walter S. Coe, assistant professor of medicine, University of Louisville, has been awarded an **A. Blaine Brower Travelling Scholarship** by the American College of Physicians. The scholarship will permit Dr. Coe to spend a month visiting a medical center of his choice.

Gordon Covell, director of the Malaria Laboratory, Horton Hospital, Epsom, Eng., will deliver the Harvard School of Public Health's **Cutter Lecture** on Preventive Medicine in Boston on May 6. The subject of the lecture will be "Current Research toward a Global Control of Malaria."

Hardy Cross, **Stratheona** professor of civil engineering, **Walter R. Miles**, professor of psychology, and **Herbert Thoms**, professor of obstetrics and gynecology, will retire from the faculty of Yale University at the end of this academic year.

Walter W. Dalitsch has been named associate director of the Northwestern University Cleft Lip and Palate Institute, effective Sept. 1. Dr. Dalitsch is a staff member of the institute and associate professor of oral surgery at the dental school.

Donald A. Davenport has been appointed chief engineer of Associated Research, Inc., of Chicago.

Benjamin M. Duggar, consultant in mycological research at Lederle Laboratories, and discoverer of aureomycin, has received the **Rho Chi Citation** at the Philadelphia College of Pharmacy and Science. Following the presentation of the citation, Dr. Duggar delivered the first annual **Julius W. Sturmer Memorial Lecture**, in which he outlined the procedures for the development of new antibiotics.

Allen S. Dunbar, formerly senior research engineer for the Stanford Research Institute, has joined **Dalmo Victor Company**, San Carlos, Calif., as assistant director of research. Mr. Dunbar previously has been a consultant to Dalmo Victor in the field of microwave optics and the theory of doubly curved antenna reflectors.

Hubert and Mable Frings, of the Department of Zoology and Entomology, the Pennsylvania State College, have been invited by the government of France to discuss with workers in a number of French laboratories, during April, May, and June, the results

of their research on biological effects of sound and radio waves on animals and on sensory physiology of insects.

George Glockler, director of the Chemical Sciences Division, Office of Ordnance Research, U. S. Army, located at Duke University, has been appointed deputy chief scientist of the OOR. Dr. Glockler is currently on leave of absence from the University of Iowa, where he has been head of the Department of Chemistry and Chemical Engineering since 1940, and has been named visiting lecturer in chemistry at Duke.

Martin Goland, associate director for engineering at Midwest Research Institute, has been reappointed to the Committee on Aircraft Construction, a technical group of the National Advisory Committee for Aeronautics. Dr. Goland will also serve another term as chairman of the Subcommittee on Vibration and Flutter, an auxiliary department of the national organization.

John F. Marchand, instructor in medicine at Cornell University Medical School, has been appointed associate professor in the Department of Medicine, University of Louisville. Dr. Marchand will serve as medical director of the new Poliomyelitis Respirator Care Center.

Frederick W. Merrifield has been appointed director of the Northwestern University Cleft Lip and Palate Institute, to succeed **John R. Thompson**, whose term as director expired in March, and who will continue with the staff.

Joseph L. Owades has joined the staff of Schwarz Laboratories, Inc., Mount Vernon, N. Y., as assistant chief chemist. Dr. Owades was recently associated with the Fleischmann Laboratories, where he was engaged in research on yeast derivatives.

Linus Pauling, chairman of the Division of Chemistry and Chemical Engineering of the California Institute of Technology, has left for a five-week visit to Europe and the eastern U. S. A visit to protein research groups in England, and attendance at the ninth triennial Solvay Congress in Brussels, beginning April 6, are included in his itinerary. Dr. Pauling will also deliver the Treat B. Johnson Lectures in chemistry at Yale, Apr. 16-21, and receive an award made to him by the Newspaper Guild of New York on April 17.

Willard B. Robinson, associate professor of chemistry, Division of Food Science and Technology, New York State Experiment Station, Geneva, has been granted a year's leave of absence to serve as technical secretary of the Food Protection Committee of the Food and Nutrition Board, National Research Council.

James K. Shafer, assistant chief of the Public Health Service's Division of Venereal Disease, has been named chief of the division, and **Clarence A.**

Smith, of Chicago, was named assistant chief. Dr. Shafer succeeds **Theodore J. Bauer**, who recently became medical officer in charge of the Public Health Service's Communicable Disease Center, Atlanta.

Rolf Strom, of the Norwegian Technical Museum in Oslo, has been granted a year's leave of absence to study and observe laboratory photographic techniques in the U. S. He will arrive in this country in May.

J. Boiscan Wiesel, assistant to the general manager of Hercules Powder Company's Cellulose Products Department, will retire Apr. 30, upon completion of 37 years with the company.

Education

A new medical college, to be erected by Yeshiva University, will be named the **Albert Einstein College of Medicine**. It will be the first unit of a \$25,000,000 medical center in the northeast part of the Bronx and the first medical school to be built in New York in more than 50 years.

Cornell University has announced a series of 10 lectures on "Current Research in Vitamin Chemistry," Apr. 7-May 7, by Karl Folkers, associate director of Merck and Co.'s Research and Development Division. Supported by the George Fisher Baker Visiting Lectureship, the series is open to the public.

The **University of Louisville** has established three new sections in the Department of Medicine: Rheumatic Diseases, Robert L. McClendon, chief; Hematology, Marion F. Beard, chief; and Endocrinology, James Robert Hendon, chief. A new chair in the Department of Internal Medicine will be known as the John Walker Moore Professorship of Medicine. The School of Medicine has also announced the establishment of the Floyd Brewer Memorial Foundation, by Walter E. Brewer, an alumnus.

The **University of Minnesota**, Department of Botany, will offer its graduate course in Ecological Plant Geography as a symposium during the spring quarter. Visiting lecturers include E. Lucy Braun of the University of Cincinnati (eastern forests), Herbert C. Hanson of the Catholic University of America (arctic, subarctic, and grassland), and, from the University of California at Berkeley, Ralph W. Chaney (tertiary forests), and Jack Major (Western forests, deserts, and semideserts).

Northwestern University announces a course in catalysis in organic chemistry, June 20-Aug. 22, in the Ipatieff High Pressure and Catalytic Laboratory. Given under the laboratory's director, Herman Pines, the course will cover the principles of catalysis, hydrogenation, dehydrogenation, isomerization, polymerization, alkylation, catalytic cracking, dehydration, oxidation, and condensation.

The **University of Texas Medical Center** will sponsor special meetings on cancer, May 15 and 16, of in-

terest to scientists and physicians. Included will be the Seventh Annual Symposium on Fundamental Cancer Research, a session devoted to selected papers on "New Methods for the Study of Cells," and the Cancer Pathology and Radiology Conference which will cover "Tumors of Lung and Pleura." The South Central Section of the College of American Pathologists will participate. The fourth Bertner Foundation Lecture will be given by Charles C. Huggins, University of Chicago School of Medicine, at the May 15 dinner, on the subject of "The Control of Human Cancer by Hormonal Methods."

Western Reserve University has received a bequest from the estate of Mrs. Gertrude Chandler Tucker to establish a "Gertrude Chandler Tucker Fund" for research pertaining to children's diseases. More than \$2,000,000 will accrue to the university, to be allotted to research, employment of personnel, and publication of research results.

Grants and Fellowships

Upon the recommendation of the Committee on Growth of the Division of Medical Sciences of the National Research Council, the American Cancer Society has awarded 253 grants-in-aid for cancer research totaling \$1,758,000. Ninety members of panels and sections of the Committee on Growth met in December to consider applications for these grants.

The American Home Economics Association reports that its international fellowship and scholarship program for 1952-53 is helping six young women from abroad to study home economics teaching methods, another young woman visitor to study nutrition and administration of school lunch programs, and still another to take courses in child development and welfare and nursery education. These annual scholarship awards are made possible through contributions of members, affiliated student clubs, and honorary home economics societies, and the cooperation of colleges and universities to which recipients are assigned. The Institute of International Education aids in selecting award recipients and in arranging their program.

The Damon Runyon Memorial Fund for Cancer Research announces that grants to institutions and research fellows have reached a total of \$6,217,859, as the fund passes the \$7,000,000 mark in contributions. All of the fund's monies go for research, none for expense. To date, awards have included 320 grants and 213 fellowships in 163 institutions in 46 states, the District of Columbia, and 14 foreign countries.

Eli Lilly and Company announces two recent grants. The first is to Oregon State College, to support Vernon H. Cheldelin, director of the college's School of Science, in research on coenzyme A. The second is to the University of Vermont, to support John E. Little of the university's Department of Agricultural Biochemistry, in his work on the isolation from plants of a substance having antibiotic activity.

The Research Corporation has made two grants in the field of chemistry. The Harvey Nathaniel Davis Grant of \$5000 in honor of the late president of Stevens Institute of Technology has been made to that school for the work of Robert B. Green, associate professor of physics, in the study of the growth and structure of oxide films. The electron microscope and electron diffraction will be used simultaneously with the objective of developing methods of producing oxide films that will protect metals from corrosion. The second award goes to Ursuline College, in the amount of \$2250, in support of a project in wax research under the direction of Sister M. Concetta, O.S.U., chairman of the college's Chemistry Department.

The St. Louis Academy of Science has awarded an AAAS Research Grant to Hampton L. Carson and W. C. Blight of Washington University, St. Louis, for their research project: "Genetics and Ecology of Natural Populations of *Drosophila*."

The Shell Oil Company has increased its 1953-54 research and fellowship program by 15 per cent, making a total of \$177,500. There will be 14 fundamental research grants and 48 fellowships. Research grants of \$5000 each are made directly to university science departments to aid in conducting basic research in chemistry, chemical engineering, geology, mechanical engineering, metallurgy-corrosion, physics, and engineering mechanics. Schools receiving fundamental research grants in 1953-54 are: Caltech, Carnegie Tech, University of Chicago, Harvard, MIT, Princeton, University of Rochester, Stanford, and Yale. The fellowship program is designed to help outstanding graduate students obtain advanced degrees in the fields of chemistry, chemical engineering, geology, geophysics, mechanical engineering, petroleum production engineering, physics, and plant science. Shell fellows are selected by their colleges or universities, and each fellow receives a stipend of \$1500 a year plus payment of tuition and fees. The school receives \$400 for related research expenses of the fellow. Candidates in their last year of doctorate study are given preference, but awards may be made to other graduate students. The fellows are under no obligation to Shell.

In the Laboratories

American Electronic Laboratories, Inc., specializing in production of electronic instruments for medical research, has announced the purchase of 48 acres of land near Colmar, Pa., on which two buildings, for research and production, are now under construction. One will be used for antenna experimentation. The other will serve as an adjunct to other high frequency experiments now being conducted.

The Eastman Kodak Company has developed improved new infrared sensitive photographic plates and films which are so fast that they will give the 200-

inch Hale Telescope on Mount Palomar as much space-penetrating power as a fantastic 900-inch would have had a year ago. The plates are designed for studies on the ultimate helium emission line at 10 829 Å, and are expected to prove of considerable value both in examining the nature of the physical universe and in studies of thermonuclear reactions involving helium.

The Los Alamos Scientific Laboratory of the University of California announces the addition to its staff of Phyllis J. Allen, Jesse E. Ashley, James H. Richardson, and Thomas F. Wimet.

The Minute Maid Corporation has honored the late Norman V. Hayes, who laid the foundation of the frozen orange juice concentrate field, by dedicating its new laboratory in Plymouth, Fla., to his memory. Dr. Hayes, a graduate of the University of California, won wartime fame at National Research Corporation as the scientist who developed the dehydration process for penicillin. The young scientist-engineer was accidentally electrocuted in 1946 while supervising the final test run of the Plymouth plant which later was to become a part of the Minute Maid Corporation.

The Research and Control Instruments Division of North American Philips Company, Inc., will sponsor a Diffraction School, Apr. 20-24. Among the participants will be I. Fankuchen, Polytechnic Institute of Brooklyn; William Parrish of the Philips Laboratories; Herbert Friedman, Naval Research Laboratory; Normal Walter of G. E.; A. N. J. Heyn, School of Textiles, Clemson, South Carolina; James I. Mueller, University of Washington; B. E. Warren and Martin J. Buerger of MIT; and Ray Pepinsky, Pennsylvania State College.

Meetings and Elections

The fourth Alaskan Science Conference, sponsored by the AAAS Alaska Division, will be held in Juneau, Sept. 28-Oct. 2. Information may be obtained from Dorothy Jean Thompson, Box 938, College, Alaska.

The eleventh annual Anthracite Conference, sponsored by the Anthracite Institute and Lehigh University, will be held May 7 and 8 at Lehigh University, with Louis C. McCabe, chief of the fuels and explosives division of the U. S. Bureau of Mines, Washington, D. C., as guest speaker. Subjects to be discussed include tobacco curing with anthracite, stoker sales and service, activated carbon, new equipment, fuel economy studies, and mechanical mining.

The fifth International Neurological Congress will take place in Lisbon, Portugal, in September. A meeting sponsored by the Spanish neuropsychiatrists in commemoration of the birth of Cajal will be held in Madrid immediately afterwards. The executive committee has selected three topics for the symposia of the congress: cerebrovascular conditions (two sessions); the parietal lobe; and metabolic diseases of the

nervous system. The individuals in charge are respectively Drs. Egas Moniz and Alajouanine, Dr. F. M. R. Walshe, and Dr. Van Bogaert.

The officers of the Congress are as follows: honorary presidents, Gordon Holmes, Georges Suillain, Dr. André-Thomas, Th. Alajouanine, Egas Moniz; honorary vice president, A. Augtregesilo; president, Antonio Flores; secretary general, Almeida Lima (Hospital Julio de Matos, 53 Avenida Brasil, Lisbon); treasurer, J. Imaginario; assistant treasurer, V. Ramos. Vice presidents so far appointed to represent various constituent countries are: Belgium, Prof. Van Gehuchten; Brazil, Deolindo Couto; Chile, A. Asenjo; Denmark, Knud Krabbe; France, Raymond Garcin; Great Britain, F. M. R. Walshe; Holland, W. Sillevius Smitt; Italy, L. De Lisi; Norway, Prof. Monrad-Krohn; Spain, J. Lopez Ibor; Sweden, Nils Antoni; Switzerland, F. Luthy; Turkey, Sukru Aksel; U. S. Henry Alsop Riley. For further information address H. Houston Merritt, Neurological Institute, 700 W. 168th St., New York 32, N. Y., or, in Canada, J. Allan Walters, Medical Arts Bldg., Toronto, Ont.

The 30th Anniversary International Petroleum Exposition, May 14-23, in Tulsa, Okla., will feature \$100,000,000 in exhibits. In addition to a comprehensive display of the equipment used today in the petroleum industry, there will be glimpses into the past and future. The 28-acre site will have 883 sheltered booths and many exhibits in the open. The Hall of Science will include displays depicting the course of oil activity from exploration through processing and distribution.

The 35th annual meeting of the Scientific Apparatus Makers Association will be held at the Greenbrier, White Sulphur Springs, W. Va., May 24-28. Lyman J. Briggs, former director of the National Bureau of Standards, and an honorary director of SAMA will receive the association's award for outstanding achievement and service to the scientific instrument industry. Bausch & Lomb Optical Company, Rochester, N. Y., celebrating their 100th anniversary this year, will also be honored by receiving a 100-year certificate from the apparatus makers and suppliers commemorating the event. The theme of this year's meeting will revolve around the industry's present and future economic position and the improvement of business methods and procedures.

More than 70 delegates from 18 territories, as well as the Kingdom of Tonga, are expected to attend this year's South Pacific Conference, to be held at the headquarters of the South Pacific Commission, Noumea, New Caledonia, in April. Problems relating to the welfare and progress of their peoples will be discussed. The agenda for the conference includes topics in three main fields: Economic development, including problems of conserving and improving the resources of the land and water: Relation of population to resources, and marketing problems: Health, including

health education of the peoples, and diet and its improvement: Social development, including role of women and women's organizations in the community, and what should a child have been taught by the time it leaves school? At the second conference a progress report will be presented on the action taken by member governments, their administrations, and the commission on the 42 resolutions adopted at the first conference in 1950 on problems of South Pacific territories in the fields of public health, social development, and economic development.

The Wyoming Geological Association will hold its Eighth Annual Field Conference at Laramie, Wyo., and vicinity on July 30, 31, and August 1. The University of Wyoming will be host and make its facilities available for conference purposes. Daily field trips will be conducted in the Laramie Basin of Wyoming,

and the North Park Basin of Colorado. Registration will be held on the campus on the afternoon of July 29th. The geomorphology of the Medicine Bow Mountain Range, the structural relations of the Medicine Bow Mountains to the Laramie Basin and the sedimentary structures of the Fountain and Casper formations of the southern Laramie Basin will be studied in the field on the first day of the conference. The second day will be devoted principally to oil fields. Essentially every oil field in the Laramie Basin will be visited, and also the world-famous dinosaur locality at Como Bluff. The last day's trip will be concerned with the structure and stratigraphy of the North Park Basin in north-central Colorado. General chairmen for the conference are S. H. Knight and Horace D. Thomas, University of Wyoming. Inquiries should be addressed to the Wyoming Geological Association, P. O. Box 545, Casper, Wyo.



The Science Library

A VALUABLE and much appreciated feature at any annual meeting of the AAAS is the Science Library, an important part of the Association's Annual Exposition of Science and Industry. Essentially, it is an exhibit of the most recent scientific and technical books, especially arranged to inform, and to be enjoyed by, the attending scientists; to provide an opportunity for publishers to show selected titles; and to aid librarians in preparing book purchase lists.

In its antecedents, the Science Library goes back many years; indeed, it is not improbable that new books have been shown by individuals at most of the meetings of the Association throughout its long history. Soon after commercial exhibits became established at AAAS meetings in the early twenties, book publishers began to rent booth space to exhibit their publications. The Science Library, operated by the Association itself, was inaugurated as an additional service, both to scientists and to publishers, at the Atlantic City meeting in 1932. It was stated then that "This [was] a premier attempt to bring all the science books of the calendar year together for inspection by the membership of the societies concerned."

In more recent years, it has been decided that titles in the Science Library need not be exclusively those of the current year. Most of the volumes entered, however, are, very largely, the latest scientific books—texts and monographs—together with the latest editions of standard reference works. These are arranged on open shelves under 21 distinct but comprehensive fields of interest—namely, aeronautics, agriculture, anthropology, astronomy, biological sciences, botanical sciences, chemistry, dentistry, economics, education, engineering, general science, geol-

ogy and geography, history of science, industrial science, mathematics, medical sciences, physics, psychology, sociology, and zoology. Additional classifications can be provided at any time, but minor subdivisions are not attempted in view of the scope, and even interdisciplinary penetration, of so many of the books.

A collection of between 500 and 1000 books from 35-50 publishers is thus displayed at each meeting. The publishers represented in any given year—some of them with their own booths in other parts of the exhibit area—include many of the large producers of scientific books, a number of university presses, and other companies with a title or two of particular interest to scientists.

The Science Library is a browsing library. Those who attend are free to consult the section of their specialty or to glance at all the titles, to remove books from the shelves, and to read in the comfortable lounge chairs and settees. Trained librarians, volunteers from the local chapter of the Special Libraries Association, are on hand to assist, if needed, and to keep the books in order.

The Science Library is one of the most popular features of the Exposition. At any hour the exhibits are open, one to twenty persons may be observed scanning titles and dipping into the contents of the new books. Although no detailed observations have been made, the typical patron probably spends more than twenty minutes in the Science Library. Beyond the opportunity to inspect the latest books of a particular scientific field, the comfortable atmosphere invites the foot-weary conventioner to rest—and it was long ago discovered that the library is a good place to arrange to meet friends.

RAYMOND L. TAYLOR

Associate Administrative Secretary

Book Reviews

Ballistics in the Seventeenth Century. A study in the relations of science and war with reference principally to England. A. R. Hall. New York: Cambridge Univ. Press, 1952. 186 pp. Illus. \$4.00.

It is easy to see why the seventeenth century, the age of the revolution in thought that first shaped our modern tendencies in science, has a special interest for historians and scientists. To what extent the new science—more especially the new dynamics of Galileo, Huygens, and Newton—can be explained in terms of the social and economic forces of the time, and how far it resulted from the internal laws of scientific progress, are questions on which students of the period are sharply divided. There have been staunch adherents of both extreme positions. A. R. Hall, assistant lecturer in the history of science at Cambridge University, makes an important contribution to our understanding of this broader question by examining closely the special case of ballistical investigations during the sixteenth and seventeenth centuries.

At first glance the influence of military technology on early science seems undeniable. It is well known that it was for the ostensible purpose of determining the ideal trajectory of a projectile that Galileo made his studies of falling bodies, developed his theory of inertia, and overthrew the structure of Aristotelian dynamics. This example has been repeatedly cited to illustrate the influence of military problems upon science. But Dr. Hall makes it clear that we have no right to infer from this an intimate collaboration between scientist and technologist; still less an immediate application of these discoveries to the useful arts. Although he would probably be the last to deny the interest of the seventeenth-century scientist in the world of practical affairs, yet he strongly insists that "men were led to discoveries in mechanics less by their practical usefulness than by the logic of historical development." The seventeenth-century contributions to ballistical science grew gradually out of the earlier tradition of scholastic speculations on motion, as modified by the impetus theory of the later Middle Ages and by the work of sixteenth-century theorists like Tartaglia. As we follow the progress of ballistical studies from Galileo through the time of Huygens and Newton, Hall demonstrates with great persuasiveness that the immediate purpose of these studies was scientific and mathematical; and that the results, since they assumed ideal conditions not encountered in nature, were not applicable to the needs of the practical artillerist. His examination of the manuals intended for the guidance of the soldier shows that, when they were not merely practical handbooks, they clung to the obsolete doctrines of the later scholastic physics long after the new dynamics had been universally accepted by men of science. No serious attempt to popularize the new parabolic ballistics was made until the last third of the century.

Dr. Hall has provided a useful corrective to some of our current notions by stressing the *de facto* independence of dynamical science from the fields of military technology and invention. But our author does less than justice to the ideology of utility that prevailed in the seventeenth century. The failure of the "new experimental learning" to find useful applications in this and that field of technology does not in itself justify treating its spokesmen as mere propagandists indulging in wishful thinking when they stress its social utility. Apart from the question whether men like Tartaglia and Galileo believed—as Hall's own evidence showed that they did—that they were making important contributions to the art of war, the fact remains that *immediate* utility and application were not what the apologists of the new learning habitually stressed. The new learning was defended as useful knowledge, first and foremost because it was the *kind* of knowledge that could be applied to the real world. Unlike scholastic natural philosophy, with which they were constantly comparing it, its concepts and theories were rooted in experience, and capable of verification by quantitative experiment. From Bacon onwards, with a vision and perception we can but admire, the apologists of this early modern science stressed not its immediate application to useful ends, but its eventual and long-term applicability. Bacon repeatedly warned against the "overhasty and unseasonable eagerness to practice" in the pursuit of this new approach to nature. This ideological position, carefully stated by the defenders of the Royal Society—we may perhaps describe it as the doctrine of the presumptive utility of abstract science—served to clothe even the most technical and abstract investigations with a mantle of social respectability.

HENRY GUERLAC

Department of History, Cornell University

Origins of American Scientists. A study made under the direction of a faculty committee of Wesleyan University. R. H. Knapp and H. B. Goodrich. Chicago: Univ. Chicago Press, 1952. (For Wesleyan University.) 450 pp. \$7.50.

A brief article in *SCIENCE* (113, 543 [1951]) has already acquainted its readers with one of the principal findings of this study: the tremendous differences in the percentage of male graduates of different colleges who later become scientists. From 1924 through 1934, the period which Knapp and Goodrich studied most intensively, the leading college in the United States was Reed College in Portland, Ore., which sent 13% of its male graduates on to scientific careers and later listing in *American Men of Science* with Ph.D. degrees, stars, or both. California Institute of Technology was next, with 7%. Then the list went down, through the University of Chicago, which was 16th with 4%, Rochester in 38th place with 3,

and on down through a total of 489 institutions, some of which had none of their graduates included in *American Men of Science*.

Origins of American Scientists is a study of the factors associated with or responsible for the great differences in productivity. Geography was important; the Midwest and Far West did better than other parts of the country. Size and character of the institutions were also important; the percentage of male graduates who become scientists was higher for liberal arts colleges than for universities and was greatest for colleges which graduate from 40 to 150 men a year. Institutions of relatively high and those of relatively low cost were less productive of scientists than were those of intermediate cost. Catholic institutions, engineering colleges, colleges in which fraternities are strong and influential, and "big name" institutions were more or less unproductive of future scientists.

In an effort to explain these and similar findings, detailed statistical analyses were made of student-teacher ratios, library facilities, and a number of other items of information; questionnaires were sent to former graduates; and Knapp spent a year in visiting a selected list of 22 colleges.

The intellectual quality of the student body helped to explain some of the differences; schools that admit freshmen who average high on aptitude tests turned out more graduates who became scientists than did other schools, but still the geographic and other differences remained. A background of middle-class, Protestant, agrarian or semiagrarian, frontier pragmatism seemed to provide a particularly congenial atmosphere for the development of scientific interests. Students from such backgrounds are particularly likely to be found in the West and in the 1924-34 period were most likely to attend nearby institutions. Those who entered schools that had become rather secularized from the strict Protestantism of their earlier years became scientists in somewhat larger numbers than did those who entered other types of colleges. So did those who attended colleges that offered a liberal education—as distinct from colleges with a more pronounced vocational emphasis. In contrast, the famous and the high-cost institutions turned out fewer scientists, partly because the students who were able to afford their higher costs were more likely to enter fields that promised greater financial rewards than a scientist can expect. A later study (see following review) indicates that the cost aspect has been greatly changed since the end of World War II.

One of the most interesting portions of the book describes the teacher and the academic standards that favor the development of scientific interests. The successful teacher is not distinguished by his intellectual competence—above, of course, some necessary minimum—or by his professional eminence, or his mastery of particular pedagogic methods, but rather by the possession of two important personal qualities: "the first seemingly related to masterfulness, demanding-

ness, vitality, and energy; the second, to human warmth and social accessibility." The authors summarize these traits by pointing out that this description fits what in psychological jargon could be called a strong father figure. Between the student and the teacher who inspires him there exists more than a purely intellectual relationship; the inspiring teacher is admired, he exercises some coercion over his students, but he is also an understanding confidant and a source of rewards for work well done. Interesting confirmation of this picture came from analyses of undergraduate departments which had started a fairly high number of students along the road to scientific careers. Relatively severe requirements for the major and relatively severe grading standards, on the one hand, and a relatively keen sense of social cohesion within the department, on the other, were all associated with high productivity. The teacher, and his department, are likely to inspire students to become scientists if the standards are high enough to weed out the poor prospects, and if, after that is done, there is a friendly and rewarding acceptance of those who show promise.

For teachers who take pleasure in starting young scientists on their careers and for educational administrators who are interested in an analysis of how academic standards, practices, and atmospheres influence students, there is much of thought-provoking interest in this book. As a more casual reason for wanting to read *Origins of American Scientists*, it includes a fascinating amount of detailed statistics on hundreds of famous and not so famous American colleges and universities.

The Younger American Scholar: His Collegiate Origins. Robert H. Knapp and Joseph J. Greenbaum. Chicago: Univ. Chicago Press; London: Cambridge Univ. Press, 1953. (For Wesleyan University.) 122 pp. Illus. \$3.00.

This study differed in two major ways from the work described in *Origins of American Scientists*: the time period was more recent—since the end of World War II—and the subjects investigated were young scholars of promise instead of the mature scientists who constituted the population of the earlier book. "Young scholars of promise" are men and women who received their bachelor's degrees since 1946 and who have since received their doctor's degrees or have been given important graduate fellowships or scholarships by universities, private foundations, or the federal government.

The two studies are similar in that both sought information on the undergraduate institutions that have been most productive of students who have gone on to work for the doctor's degree. The institutions which, since 1946, have turned out a high proportion of students who have gone on to graduate work in the sciences differ considerably from those which had the highest productivity indexes twenty to thirty years ago. The top 20 institutions of the recent period in-

clude 7 that were among the top 20 in the earlier period and 13 that have come up from somewhat lower ranks. More important than the changes in relative position of individual institutions are the changes in characteristics of the institutions that are most productive. Since 1946, liberal arts colleges, universities, and technological institutes of comparatively high cost have trained relatively more students who continued into graduate work in the sciences than have those of lesser cost. Twenty to thirty years ago schools of moderate cost were more productive than either the cheapest or the most expensive ones. Since 1946 schools in New England and the East North Central region have been the most productive ones. In the earlier period those in the Middle West and Far West were the most productive. A feature which stands out as characteristically in the recent period as it did earlier is the higher productivity of private nondenominational colleges and universities over public and denominationally controlled ones.

The GI Bill, the establishment of much broader scholarship programs by some of the older and wealthier (and incidentally Eastern and more expensive) institutions, and the higher general income level, the authors suggest, have probably been responsible for the shift in the center of gravity from Midwestern and Western institutions of moderate cost to Eastern institutions of higher cost.

Unlike the earlier book, this one also considers the undergraduate origins of young scholars in the humanities and social sciences. In general, the trends in these two areas are similar to those that have been mentioned for science students. Inclusion of information on all three areas, however, permits some interesting comparisons. Some of the schools which are most productive of future scientists also rank high in the undergraduate preparation of future social scientists and humanists. In fact, there are 8 institutions that rank among the top 20 in productivity in all three areas: Swarthmore, Reed, Chicago, Harvard, Oberlin, Antioch, Carleton, and Princeton.

One of the most challenging problems posed by the findings of this study is the relatively small number of undergraduate institutions which send significant numbers of their students into graduate work in the scientific and scholarly fields. Writing of the young natural scientists, Knapp and Greenbaum report that only some 60 institutions show "significant and impressive rates of production, while among the remainder the dedicated young scholar is a rare exception among their graduates." Students who have gained distinction in graduate work in the social sciences come from a somewhat smaller undergraduate base, and those in the humanities from a still smaller base and one that is sharply concentrated in the northeastern section of the country. It is good that there are institutions which stand out well above the level of intellectual stimulation of the average American college, but how far is it desirable to concentrate the source of graduate students in a small number of

undergraduate institutions and to have those as geographically concentrated as has been the case since 1946?

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Astronomy

The Expansion of the Universe. Paul Coudere; trans. by J. B. Sidgwick. New York: Macmillan, 1952. 231 pp. + plates. \$6.00.

This highly readable book gives both the professional astronomer and the interested layman an account of the various data which led to the conception of an expanding universe. The book was awarded the Paul Pelliot Prize and the Henri de Parville Prize of the Académie des Sciences in 1950 and is excellently translated by J. B. Sidgwick.

After three introductory chapters dealing, respectively, with the observational data of our universe, the measurement of astronomical distances, and the distances and recession of galaxies, the author discusses in Chapter 4 the non-Euclidean space-time of general relativity. In Chapter 5 he treats the cosmological problem, in Chapter 6 the static universe of Einstein and the pseudo-static universe of de Sitter, and in Chapter 7 the expanding universe. Chapter 8 is devoted to a discussion of indications of a singular state of affairs about 4-5 billion years ago.

The book is very well written and can be read with profit by the interested layman—provided he is willing to read thoughtfully—and it can be highly recommended.

There are a few minor points where, in my opinion, the book could be improved. The source of none of the excellent photos of galaxies is given. The Hertzsprung-Russell diagram is referred to as the Russell diagram (p. 39). The light deflection during an eclipse is *not* in accordance with the present predictions of general relativity and can scarcely be taken as a support for the theory of general relativity (p. 120).

Coudere goes to great length to prove that Lemaitre's model fits the observational data, and he presents a strong case in favor of this. It is therefore to be regretted that the way in which he attacks other explanations is so often unnecessarily pugnacious, and even sometimes slightly malicious. This is the more surprising as Coudere mentions Omer's results of calculations regarding a nonhomogeneous model of the universe. From Omer's calculations one sees that the introduction of even a slight inhomogeneity can alter various results very considerably, and it seems to me that one must be extremely careful in adhering too rigidly to results obtained from a homogeneous model.

The case for the existence of a hyperdense state of the universe about 4 billion years ago has, to my mind, not been made. I agree immediately that all the evidence of Coudere's Chapter 8 points to a short time

scale, but this does not necessarily involve a hyperdense state. The work of Hoyle, van Albada, and Klein and collaborators has shown, so far as I can judge, that nuclei of high atomic weight could also have been formed in essentially present-day stars, without making an over-all high density necessary.

These objections are minor, and one can only be grateful to Coudere for presenting us with such a colorful account of this subject.

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Physics and Mathematics

Cloud Chamber Photographs of the Cosmic Radiation. G. D. Rochester and J. G. Wilson. New York: Academic Press; London: Pergamon Press, 1952. 128 pp. + plates. \$10.80.

Professors Rochester and Wilson, of Manchester, England, have been prominent in the development and exploitation of the cloud chamber technique. In the present volume, they give a well-rounded selection of photographs obtained in cosmic ray laboratories all over the world. Explanatory captions are included, but it is characteristic that the elementary principles of cloud chamber operation and particle behavior are not outlined. Indeed, the language of the captions will be meaningful only to those readers well initiated in cosmic ray research.

This is not so of the pictures, however. They give a refreshing sense of reality to the existence and wonderful behavior of elementary particles possessed with fantastic energy; and persons in many walks of life may be led to feel a vivid appreciation of the extraordinary phenomena portrayed. Such visual comprehension is of great value to research workers, but it is not to be decried in the form it takes in those less fully informed. Even the feeling engendered that one has actually seen cosmic rays, is not unjustifiable, for one senses all objects with essentially the same sort of indirectness.

To a student of cosmic rays a careful reading of the captions, with frequent reference to the pictures under discussion, will be thoroughly rewarding. Mental exercise is required, since the explanations are necessarily abbreviated; but in the interpretation of the pictures here displayed, almost all the known laws of behavior of high energy particles and the properties of practically all the known particles are illustrated and put to use. Thus, following the arguments of interpretation can be of as much educational value as, for instance, the study of the recorded games of experts is to the student of chess.

Many of the pictures were apparently selected for their historical interest. These include the earliest photographs of cosmic rays by Skobelzyn, those of cascade showers by Blackett and Occhialini, the photograph by Anderson, credited with the discovery of

the positron, some of the earliest records of mesons and their decay, and the first pictures of V-particles by Rochester and Butler. Such examples are also of educational value, because they demonstrate so effectively the process of discovery.

Other photographs are illustrative of the processes of interaction and transformation that have been studied in the cosmic rays, special emphasis being given to phenomena under current investigation. There are also pictures illustrating the distinctive properties by which different particles may be recognized. And another group of pictures was selected to show characteristics of the technique of cloud chamber operation, together with some of the many variations of apparatus and technique that have been employed for special purposes.

In almost all the commentaries, attention has been called to the technical excellence, or imperfections, of the pictures and the influence of the quality on the interpretation that can be made.

The attention given to the limitations on interpretation in case after case throughout this book leaves one at the end with the feeling that even the cloud chamber, which provides more detailed information about individual cosmic ray events than any other instrument (except perhaps the photographic emulsion), is still so severely limited that the pathway to further knowledge is very hard. For the student this emphasis on the limitations, as well as the powers, of the instrument is essential to provide a proper balance.

In many respects the virtues of the cloud chamber and the photographic emulsion in cosmic ray research are complementary, and both techniques share the virtue of pictorial clarity. In the field of publications, an obvious need is to supplement this fine collection of cloud chamber photographs of cosmic radiation with a similar book exhibiting photomicrographs of representative cosmic ray events observed in photographic plates. The summaries of the interpretations that might accompany such pictures would be of educational value equal to those in the present book and the duplication of subject matter would be so little that every teacher and researcher in high energy physics would want copies of both books on his desk.

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High-Energy Particles. Bruno Rossi. New York: Prentice-Hall, 1952. 569 pp. Illus. \$12.50.

"If the past is any guide, theory will probably continue to lag behind the observational approach," says a recent research review. Must the same be said of elementary particle physics? This subject engrosses the greatest concentration of talent in theoretical physics ever seen. All minds work to open the secret door, to reveal the glittering central mechanism, to comprehend in one view the stability of the half-dozen or so elementary particles (Table 1), and the processes which

TABLE 1
ELEMENTARY PARTICLES AND ELEMENTARY FIELDS FOR THE TRANSFER
OF ENERGY BETWEEN PARTICLES

	Name	Mass at rest relative to electron	Charge relative proton	Spin	Spontaneous transformation	
Particles	Nucleon	{ proton (p)	1836.14	1	$\frac{1}{2}$	Stable
		{ neutron (n)	1838.65	0	$\frac{1}{2}$	$n \rightarrow p + e^- + \bar{\nu} + 0.77 \text{ mev}$ (12.4 min)
	Electron	{ positron (e^+)	1	1	$\frac{1}{2}$	Stable
		{ negaton (e^-)	1	-1	$\frac{1}{2}$	"
	Meson	μ^\pm -meson	209	± 1	$\frac{1}{2}$	$\mu \rightarrow e + \nu + \bar{\nu} + 107 \text{ mev}$ ($2.1 \times 10^{-6} \text{ sec}$)
		π^\pm -meson	276	± 1	0	$\pi \rightarrow \mu + \nu + 33 \text{ mev}$ ($2.65 \times 10^{-8} \text{ sec}$)
		π^0 -meson	266	0	0	$\pi^0 \rightarrow \gamma + \gamma + 136 \text{ mev}$ ($\sim 3 \times 10^{-14} \text{ sec}$)
		τ -meson	977	± 1	0?	$\tau^+ \rightarrow \pi^+ + \pi^0 + \pi^- + 77 \text{ mev}$ ($> 5 \times 10^{-10} \text{ sec}$)
		V_0^0 -meson	2200	0	$\frac{1}{2}$	$V_0^0 \rightarrow p + \pi^- + 46 \text{ mev}$ ($\sim 5 \times 10^{-10} \text{ sec}$)
		Other mesons				
Quanta: mechanisms for the transfer of energy	Electromagnetism:		0	1	Stable	
	photon (γ)					
	Gravitation:		Propagated with speed of light;	0	1	"
	graviton		zero rest mass	0	$\frac{1}{2}$	"

create and destroy them. Yet theory continues to lag behind discovery. Every year, on the average, sufficient observations and cases are collected to establish the existence of one new particle. Each new entity has properties that to some minds were new and unexpected, to others appear as random drawings from an infinitude of possibilities.

The methodology for discovery and investigation of the elementary particles, the tools—both experimental and theoretical—that have put the experimentalist so far ahead of the analyst, form the content of this outstanding book by Professor Rossi, of MIT. Well known to most physicists is the sight of this leading cosmic ray investigator opening his brief case to draw out a graph, formula, or table in answer to a colleague's question. These carefully prepared compilations of data and theory now become available to all. Typical examples include: (1) A graph of absolute intensities of the principal components of the cosmic radiation as a function of depth in the earth's atmosphere—useful data at a time when we are showered with new particles more richly from outer space than from accelerators yet a-planning. (2) A collection of the principal well-established formulas for the probabilities for a fast particle to collide with an electron and to radiate, or to undergo, elastic scattering as it passes through the field of force of an atomic nucleus—all these formulas being presented for the several spin values that may occur. The likelihood of these elementary processes depends upon spin of the particle in question, a useful circumstance in the interpretation of the observations. (3) Extensive graphs

and tables for the loss of energy of fast particles via ionization in air, carbon, aluminum, iron, and lead. (4) Thorough treatment of the theory of production of pairs of positive and negative electrons by energetic photons, and radiation of new photons by electrons, and of the theory of shower multiplication based on these two processes. (5) Attractively prepared diagrams of the construction and operation of the principal measuring devices, with remarks on their efficiency, reliability, and sensitivity. (6) Extensive tables and graphs on the properties of photographic emulsions and their use in measuring energy, mass, and other characteristics of elementary particles. (7) Summary of much of the existing evidence on the properties of π - and μ -mesons, the principal omissions being only the extensive observations made at Chicago on nuclear scattering of π -mesons in the past few months and the measurements by Keuffel of time for capture of a μ -meson into an atomic nucleus. (8) Comparison between theory and experiment on the electromagnetic interactions of high energy particles (pair production, photon absorption, photodisintegration of the nucleus, ionization, shower production)—all of which goes far to establish the satisfactory status of this part of the subject. (9) Compilation of existing information on the collision of two high energy nucleons, and of the interaction of one such nucleon with a complex nucleus (production of multiprong stars by nucleon emission; production of mesons).

The last chapter constitutes more than a quarter of the book and deals with the part of the subject that

is in rapid development and to which Rossi has made important contributions: nuclear interactions of the cosmic rays. It begins with 40 beautiful and instructive cloud chamber and emulsion photographs of cosmic ray events of most varied character. There follows in tables and graphs an analysis of the statistical data on the particles which emerge from nuclei on bombardment with particles of energies of the order of 10^9 ev. The book concludes with an analysis of the propagation of nucleons through the atmosphere.

This book might well be given an alternative title: "The Elementary Processes of High Energy Physics." In terms of these elementary processes it is appropriate to think of a comprehensive interpretation of cosmic ray phenomena. A second book on this subject is promised by the author. If it is as thorough as the present contribution, an extraordinarily good basis will have been established for further attack on the elementary particle problem.

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Bessel Functions: Functions of Positive Integer Order, Part II. British Association for the Advancement of Science, Mathematical Tables, Vol. X. Prepared on behalf of the Committee for the Calculation of Mathematical Tables by W. G. Bickley et al. New York: Cambridge Univ. Press, 1952 (for the Royal Society). 255 pp. \$11.00.

This volume, the last of the series of British Association Mathematical Tables, continues the tables given in Volume VI of the series (*Bessel Functions: Part I: Functions of Order Zero and Unity*) to functions of positive integer orders up to 20, for various values of argument up to 25 for the J and Y functions, and up to 20 for the I and K functions.

Although this volume has been included in the British Association series, it has been published under the auspices of the Royal Society, whose Mathematical Tables Committee replaced the British Association Mathematical Tables committee in 1948 while this volume was in preparation.

The first 50 pages include, in addition to a preface which lists the members of the committees responsible for the tables, a description of the tables, and a short account of the methods used in calculating and checking them. This section will be of special interest and value to computers. In a section entitled "Functions and Formulae," a list of formulas relevant to the functions tabulated is given. This list is quite comprehensive and greatly enhances the value of the volume. A section on interpolation, showing how values of the functions for arguments between those tabulated may be calculated, is included in the preliminaries. For the 8-figure tables, standard methods, using second differences therein printed, are recommended. Other methods, not depending on differences, and which may be used in the 10-figure tables, are also described.

In recent years two important tables of Bessel func-

tions have appeared, those of Enzo Cambi (Dover Publications [1948]), and the Harvard Tables (Harvard Computation Laboratories, Harvard University Press [1947-51]). In both tables, however, only functions of the J type are given, whereas here almost as much space is devoted to each of the functions of the other three types, Y , I , and K . The following tables are given:

Table I. $J_n(x)$, $n = 2(1)20$, $x = 0(0.1 \text{ or } 0.01)10(0.1)25$; 8 decimals.

Table II. $Y_n(x)$ or $y_n(x)$, $n = 2(1)20$, $x = 0(0.1 \text{ or } 0.01)10(0.1)25$; 8 figures.

Table III. $I_n(x)$ or $e^x I_n(x)$, $n = 2(1)20$, $x = 0(0.1 \text{ or } 0.01)10(0.1)20$; 8 figures.

Table IV. $K_n(x)$ or $e^{-x} K_n(x)$, $n = 2(1)20$, $x = 0(0.1 \text{ or } 0.01)10(0.1)20$; 8 figures.

Table V. $J_n(x)$, $n = 0(1)20$, $x = 0(0.1)25$; 10 decimals.

Table VI. $Y_n(x)$, $n = 0(1)20$, $x = 0.1(0.1)25$; 10 figures.

Table VII. $I_n(x)$, $n = 0(1)20$, $x = 0(0.1)20$; 10 figures.

Table VIII. $K_n(x)$, $n = 0(1)20$, $x = 0.1(0.1)20$; 10 figures.

The first four of these tables provide central second differences or modified second differences for interpolation purposes.

The arrangement and printing are excellent; the reviewer has checked a number of entries chosen at random and has found no misprints.

Bessel functions enter into the mathematical solutions of a wide range of scientific problems. The information contained in this volume will therefore be welcomed by, and will greatly assist, many workers in the various fields of science and technology.

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Theoretical Nuclear Physics. John M. Blatt and Victor F. Weisskopf. New York: Wiley; London: Chapman & Hall, 1952. 864 pp. illus. \$12.50.

This excellent book contains a comprehensive treatment of the principal and more important applications of theoretical viewpoints to nuclear physics. It was written at the termination of the period of independent development of classical nuclear physics—i.e., nuclear physics making no use of meson phenomena. It is probable that the next book of comparable thoroughness will be greatly affected in viewpoint and emphasis by the rapid strides that are being made in the field of meson and other high-energy researches, and that its outlook will be appreciably different. The authors and the publishers have contributed to the maintenance of permanent values in science in providing a record of ideas at the approximate termination of the era in which neutron-proton- β information was developing independently of the newer physics.

Blatt and Weisskopf place their main emphasis on the two-nucleon problem, the resonance theory of nuclear reactions, general requirements that can be set on nuclear forces from empirical evidence, multipole electromagnetic radiation, β -ray theory, and shell theory. The mathematics of angular momenta and electromagnetic radiation are discussed in the addenda. The main approach is physical, mathemat-

cal detail being subordinated. There are many carefully prepared diagrams and tables facilitating absorption of the material. The authors' preferences have led to the elimination of some items, such as the theory of hyperfine structure and spectroscopic isotope shift—presumably on the grounds that the atom may be considered as an instrument in the exploration of the nucleus. The theory of stopping power of charged particles is not included. Similarly, the older literature on the 3- and 4-body problems, although quoted, is not discussed in great detail.

It is a pleasure to see moderation regarding a subject so close to Weisskopf's heart as Bohr's "compound nucleus-continuum theory" and caution regarding the fact that, in some cases, experiment indicates the applicability of the independent particle approximation. The probable necessity for the inclusion of surface tension terms in Wigner's uniform density model has not been forgotten. It is reassuring to see a whole section prefaced by the statement "It is possible that the developments of this chapter will soon be obsolete," as has been done in the discussion of saturation of nuclear forces. Indeed it appears increasingly probable that the many body forces that the authors have in mind will become the fashion, the π -meson having been shown to be pseudoscalar.

The reader of a book will always find a few points that he would have discussed differently. The view that high energy n - p and p - p scattering speaks on the whole against charge independence is possibly paying too much attention to the work of Christian and Hart, and of Christian and Noyes, and too little to that of Jastrow, and of Case and Pais. The last two investigations are mentioned, but the pessimism regarding charge independence remains. The recent work of Lévy with the pseudoscalar meson theory appears to strengthen the view of charge independence by providing an explanation for the repulsive core used by Jastrow. In connection with the treatment of the "effective range" the reviewer would have preferred not to single out $E=0$ as necessarily the point around which the expansion should be made. The authors have not completely succeeded in avoiding partiality to the Cambridge mass nuclear physics scene, but the vast accumulation of literature in nuclear physics makes it difficult, of course, not to favor the work among one's acquaintances.

The writer has no enthusiasm for standardization of scattering information in terms of "effective range" and "intrinsic range." The effective range singles out the relative energy $E=0$, which has no better standing than other values of E . The last International Conference on Classical Nuclear Physics at Chicago showed the type of confusion that can arise. The pedagogic value of a presentation of general features is not denied, but for a clear specification of trial potentials the relatively dull, uninspired but unambiguous specification of an interaction term in the Hamiltonian can hardly be improved on.

The reviewer has been especially impressed by the

discussion of resonance theory of nuclear reactions. He believes that the authors have emphasized most of the truly essential features of the situation. The chapters on electromagnetic radiation, β -ray theory and shell theory, interrelated through the empirical material, are welcome parts of the survey, combining simplicity of presentation with reasonable exhaustiveness of essential facts. The chapter on β -ray theory could have been more extensive, with more discussion of orbital capture in the light of the work of Marshak and with more attention to the mathematical formalism in general. On the other hand, there are references to most of the relevant papers.

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Theory of Matrices. Sam Perlis. Cambridge, Mass.: Addison-Wesley, 1952. 237 pp. \$5.50.

In this volume Dr. Perlis has attempted to put before mathematicians and nonmathematicians a great deal of useful material on matrices, together with detailed proofs and numerous illustrations and problems. Beginning with rectangular arrays and the usual matrix operations and with a discussion of vector spaces (vectors being matrices of one row or column) and the facts about determinants to the theory of congruence, Hermitian congruence, and similarity of matrices, he proceeds to the study of matrices with polynomial elements, of orthogonal matrices, of characteristic roots of matrices, and of linear transformations. Bilinear and quadratic forms are considered in terms of their matrices.

Despite minor errors and causes for irritation, the author is eminently competent to write this book and he has obviously devoted much time and thought to it. I am convinced, however, that he has written the wrong book. Since the same could be said with equal force about other recent textbooks on this subject, it seems worth while to make my point in detail.

The twentieth century might be called a golden age of mathematics. Great advances have been made in many directions. The one that concerns us here, and the one that carries on a great tradition beginning with the geometry of Euclid, is the emphasis upon abstraction for the purposes of unity and simplicity and clarity. The useful principle that has been at work through the years may be illustrated by a trivial observation: If our subject is forests, we cannot but be aware that forests are made up of trees, many and diverse; for some purposes we may want to consider trees in detail, but for others we should ignore the trees and think of the forest. The constant effort to keep focused on the main object has exerted a steady pressure upon the form of the subject matter now before us. During most of the nineteenth century determinants played an important role in algebra, and arrays of numbers occurred only incidentally. In the early part of the present century arrays assumed a new aspect, as matrices, and began to be studied vigor-

ously, particularly after the fundamental papers of Wedderburn in the first decade. The reign of the matrix was clearly at its height when MacDuffee's *Ergebnisse* tract appeared in 1931. It was then apparent that determinants had assumed a lesser place and were to be considered as occasionally useful, sometimes essential, but best avoided whenever possible. It is my conviction that, in 1953, matrices too must be regarded as of subsidiary importance.

When geometry was enriched at the hands of Descartes by the introduction of coordinates, it was also impoverished in a way that has now become quite clear. Coordinate systems, after all, are always special, and must be chosen with great care if they are not to complicate the problem under consideration. This point is well illustrated by the vector of physics. Such a vector is not a set of three components but an entity in its own right, and much can be gained by acting on this truth. Similarly, in connection with the general notion of a vector space, which has application to many widely separated parts of mathematics, vectors should not be considered as sets of numbers. Here is the first point on which many active workers in modern algebra, topology, and analysis will be inclined to take issue with Perlis. I am equally sure that the mathematical layman, when introduced to vector spaces in abstract terms enriched and illustrated by diverse concrete examples and problems, would find himself impatient with the older points of view. The matrix would then turn up as a subsidiary notion in two different connections: (1) as the set of "components," with respect to specified bases, of a linear transformation from a vector space into a (possibly different) vector space; (2) as the set of values of a bilinear form corresponding to the basis vectors. The reduction of a matrix to a canonical form would be considered as simply an alternative and somewhat awkward approach to the problem of selecting a basis of a vector space (or bases of two vector spaces) connected in an intrinsic way with a given linear transformation or a given bilinear or quadratic form. The determinant of a matrix could not and would not be neglected, but it could be presented as the determinant of a linear transformation, or as a relative invariant of a bilinear form, or as a skew-symmetric n -linear form on an n -dimensional vector space. The object would be not to overwhelm or to confuse, but to put first things first. And the gain in unity, in simplicity, in clarity, and in power would be immense.

The above remarks are directed not to Dr. Perlis, who certainly needs no introduction to modern algebra, but to the many mathematical laymen who are finding matrices useful in their work. Elementary textbooks on matrices are being written in answer to the demand of a wide public; a better-informed demand would encourage the same writers to produce even more useful books.

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Ferromagnetic Properties of Metals and Alloys.
K. Hoselitz. New York: Oxford Univ. Press, 1952.
320 pp. Illus. \$8.00.

This book is a valuable contribution to the existing literature on ferromagnetism—particularly for the reader who is interested in the subject from a metallurgical, rather than a purely theoretical, point of view. The book takes up the magnetization curve in some detail, reviewing the various processes that take place in a magnetizing field and summarizing results and conclusions applicable to both magnetically hard and magnetically soft materials. A chapter is devoted to metals and alloys and the preparation of specimens for test. Another is devoted to magnetic analysis and the information regarding the condition of a test specimen obtainable from magnetic data.

In its 300-odd pages the book gives a good first look at the main areas of current interest. It is not exhaustive in its review of fundamentals, nor encyclopedic in its review of detail. It is readable, it gives many useful references, and it is up to date, particularly in the spheres of special interest to its author.

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Structure of Metals: Crystallographic Methods, Principles, and Data. 2nd ed. Charles S. Barrett.
New York-London: McGraw-Hill, 1952. 661 pp.
\$10.00.

In this second edition Dr. Barrett has retained the original logical outline and concise readable style for which his first edition was distinguished. Many sections are devoted to elementary fundamentals, and this book will serve as an excellent introductory text on crystallography and x-ray metallography for senior and first-year graduate students. The omission of problem sets appears to detract somewhat from the value of the book as a student text. No course in crystallography or x-ray metallography, however, is entirely satisfactory without the inclusion of a judiciously formulated series of laboratory exercises. And such exercises, giving emphasis to the topics of greatest importance to the instructor's objectives in his specific course, are readily based on the context and very valuable appendices in this book.

The coverage of techniques, especially those for x-ray metallography, is quite complete. One chapter is devoted to electron diffraction techniques. All sections are amply documented with references, thus giving the more enterprising students additional stimuli and ready access to the more detailed analyses provided by the original literature.

Barrett's book also serves as an important and ready reference for research investigators and experts in the field. It contains a rather complete discussion of the more advanced techniques, as well as analyses and interpretations of the existing data, on such topics as electron theory of metals, plastic deformation, dislo-

cation theory, structure of cold-worked metals, preferred orientations, anisotropy, and phase transformations. Many of these sections can be adapted for more advanced graduate student instruction.

Many revisions were made in this new edition, and the context of the first edition was increased by over 90 pages. The major changes were made on sections dealing with dislocations, imperfections, creep, structure of metals, textures and preferred orientations, precipitation hardening, and phase transformations—necessitated by the additional knowledge acquired in these fields since the publication of the first edition in 1943.

As it is now revised, Barrett's book is the most inclusive and up to date single reference that is available on the structure of metals.

JOHN E. DORN

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Chemistry and Biochemistry

Chemistry of Carbon Compounds: Aliphatic Compounds, Vol. I, Pt. B. E. H. Rodd, Ed. Amsterdam-Houston: Elsevier, 1952. 684 pp. \$17.50.

This book is a continuation of the excellent work which was published in 1951 (*SCIENCE*, 116, 181). The style is essentially the same as that used in the last half of Part A. The tremendous task of editing this treatise was made easier by the distinguished advisors: Sir Robert Robinson, J. W. Cook, R. D. Harworth, Sir Ian Heilbron, E. L. Hirst, and A. R. Todd.

There are 13 authors of the 12 chapters, several of whom have contributed to more than one chapter. The principal classes of aliphatic compounds covered are monocarboxylic acids and carbonic acid, dicarboxylic acids, polyhydric alcohols and their derivatives, complex carbohydrates, proteins, and enzymes. A particularly extensive treatment is given to polyhydric alcohols, covering 275 pages. Generally, methods of preparation and properties characteristic of the class of compounds are presented briefly at the outset of the chapter. This is followed by methods of preparation and properties of individual members of the class of compounds and derivatives.

This reference work will fall between such widely used treatises as Richter, Karrer, and Gilman, and the more extensive Beilstein and Elsevier in degree of treatment of the subject matter. The style of presentation by classes and compounds within classes is similar to Beilstein.

It appears that this will be an invaluable series of books for the organic chemist. The treatise is far too voluminous for a textbook; it will find its widest use as a reference book. The subject matter is up to date and is presented in a modern, comprehensive manner. It is strongly recommended for every qualified organic chemist.

E. T. MCBEE

Chemistry Department, Purdue University

Metabolic Maps. Wayne W. Umbreit. Minneapolis: Burgess Pub., 1952. 439 pp. Illus. \$6.00.

From the introduction to the appendix, the author endeavors to present a summation of our present knowledge of the mechanisms of cellular metabolic functions. For the purposes of presentation, this objective is exceptionally well accomplished by dividing cellular biochemistry into logical subdivisions and, in workbook fashion, covering each individual reaction by means of a series of nicely executed charts, together with carefully selected references for each of the reactions diagrammed. The references include review articles, the historically important papers, and the most pertinent of the 1950-51 literature.

An excellent balance among the general fields of metabolism is achieved by allocating the first five chapters to the subject of carbohydrate oxidation and its related phosphate metabolism; Chapter 6 to oxygen transfer; Chapters 7 through 10 to nitrogen metabolism, including methylation (Chapter 9) and ring-containing amino acids (Chapter 10); Chapter 11 to nucleic acids and their derivatives, and Chapter 12 to the lipids, including the latest in the enzymatic synthesis of biologically important steroids.

The make-up of the book, with its inclusion of blank space for additions and changes as they appear in the literature, permits the reader to use it in accordance with the author's suggestion as "a device for the orderly assembling of useful contemporary information without employing extensive files."

The author has endeavored to help the reader keep abreast of the broader fields of cellular metabolism. An example of how he does this and, at the same time, gives the book practical value to readers unacquainted with a specific field, is the cryptic but clear section devoted to the nomenclature of steroids. Likewise, a few pages and references on the subjects of purine and pyrimidine antagonists will be found.

The subject index is complete and especially valuable in that many of the chemical substances involved as intermediates in the various metabolic pathways are listed.

It is the opinion of the reviewer that this book will be useful, not only to those active in the field of cellular metabolism, but also as a general source of information to all those interested in modern biology.

EDWIN E. HAYS

The Armour Laboratories, Chicago, Illinois

Chemistry of the Metal Chelate Compounds. Arthur E. Martell and Melvin Calvin. New York: Prentice-Hall, 1952. 613 pp. Illus. \$10.00.

Recent years have witnessed a great resurgence of interest in inorganic chemistry, as is evidenced by the fact that the demand for inorganic chemists in industrial and academic institutions now far exceeds the rather modest supply. Although all areas of inorganic chemistry have enjoyed this rapid growth, the metal complexes have attracted particular attention, and

many groups of workers, both in America and abroad, are doing research in the field. Most of the current work involves physical chemical methods and measurements, but there is still great need for research on the preparative and descriptive side; some excellent work is being done in this area, however. The publication of this volume by Martell and Calvin is welcome—for more than 30 years has elapsed since the appearance of an authoritative book on complexes. Both authors have done excellent and extensive research on chelate compounds and are thoroughly familiar with the literature on complexes. They have surveyed it carefully and critically, and their book is well organized and clearly written. Since metal complexes are encountered in every branch of chemistry, this book should find wide use.

The metal chelate compounds differ from other complexes in containing ring structures which are formed by the union of the metal ion with two or more atoms of the same coordinating molecule. These rings give the complex peculiar stereochemical properties and greatly enhanced stability, so the chelates merit special attention. Although Martell and Calvin have been concerned primarily with the chelate complexes, they have of necessity surveyed the chemistry of all types of metallic complexes, particularly the stability constants, bond types, and structures. They perhaps overemphasize the distinction between chelates and other complexes, but in a book on chelate compounds this is excusable.

The last half of the volume deals primarily with applications of chelation, particularly in catalysis in biological systems, in analytical chemistry, in water softening, and in the dyeing of fabrics. Other authors would doubtless have emphasized other applications (e.g., electrochemistry, photography, and leather tanning), but the ones selected are representative and have received more attention than the others. The appearance of this monograph may well stimulate work along new lines and suggest new applications. The thoughtful reader will find that it answers many questions, but that it suggests far more than it answers.

JOHN C. BAILAR, JR.

*The William Albert Noyes Laboratory
University of Illinois*

A Guide to Filter Paper and Cellulose Powder Chromatography. Tudor S. G. Jones, J. N. Balston, and B. E. Talbot. New York-London: H. Reeve Angel, 1952. 145 pp. Illus.

Paper Chromatography: A Laboratory Manual. Richard J. Block, Raymond LeStrange, and Gunter Zweig. New York: Academic Press, 1952. 195 pp. Illus. \$4.50.

In chemistry, as in doubtless all other areas of science, from time to time there come about fructifying new discoveries or inventions of technique that are followed by a seemingly explosive exploitation as each new tool is applied for the re-examination of current

concepts and for the investigation of new problems, now possible or facilitated.

Paper chromatography, although not without antecedent application, is essentially a development of the past decade, deriving from the work of Martin and Synge, winners of the Nobel prize in chemistry for 1952. The rapidity of development of this general technique and of its extension in so many directions is happily recognized by the appearance of the two books reviewed here.

The term chromatography, introduced for the separation of plant pigments by selective adsorption on columns, has come to refer also to the resolution of uncolored materials by the same or analogous procedures. Paper chromatography refers to the separation of mixtures of small amounts of substances placed on an originally restricted area on the paper, as a solvent or mixture of solvents is caused to flow past. A variety of appropriate methods is applied for the location of the individual constituents thus resolved. The ratio of the distances traveled by a substance and by the solvent front, the R_F value, is characteristic of the substance under the prevailing conditions, and is of aid in identifying an unknown constituent. If the R_F value is low, it is on occasion convenient to employ R_X , the ratio of the distances traveled by the substance and by some reference substance X .

The separation of mixtures on filter paper may involve selective adsorption, ion exchange, or partition between solvents, or—not uncommonly—a combination of these. Water, and presumably certain other solvents, by hydrogen-bonding with the cellulose, apparently form what is essentially a stationary quasi-liquid phase, between which and the flowing solvent mixture a partition equilibration occurs. Knowledge of factors affecting partition coefficients serves as a basis to explain or predict results in many instances. It has not been possible, however, to develop a rigorous mathematical treatment of the process or processes involved, although the theoretical discussions in papers by Martin and associates and others offer considerable guidance. The choice of conditions is still largely on an empirical basis, although work with new compounds may be guided by what is known of the action of related substances.

The book edited by Dr. Jones has been published by H. Reeve Angel and Co., Ltd., and W. & R. Balston, Ltd., distributors and manufacturers, respectively, of Whatman filter papers and cellulose powders. Part I, a general discussion of methods and materials, includes a brief outline of the processes involved in the conversion of cotton fiber to paper and relates the nature of the slightly altered cellulose molecules and of trace substances also present to the properties that may be of concern in paper chromatography. A description of pertinent characteristics of a number of their own papers is included.

The introduction to Part II on applications outlines the influence of the nature of the solute on separations, possible modifications of the paper for special

purposes, and methods for location, identification, and quantitation of materials on paper or cellulose powder columns. The remainder of the book is devoted to numerous classes of substances, such as amino acids, sugars, phosphoric esters, purines, pyrimidines and nucleosides, vitamins, antibiotics, alkaloids, steroids, radioactive tracers, proteins and enzymes, and various other organic and inorganic substances. There are also brief sections on the use of paper impregnated with alumina or silica, and of acetylated paper, and on reversed-phase paper chromatography and on electrophoresis on paper. The sections include much interesting information, generalizations, and critical comments, but essentially no detailed description of procedures, for which the reader is directed to the references included with each section. There are an author index and a classified list of recent articles. A short section of pictures and a two-dimensional "map" for amino acids and related substances are included.

The book of Block, LeStrange, and Zweig covers essentially the same territory but is intended as "a practical manual in which tried and proven procedures, employing relatively simple equipment and available reagents, are summarized."

A chapter on theory, with some mathematical treatment, and chapters well illustrated with diagrams and pictures on general and quantitative methods are included. The chapters dealing with various classes of materials include descriptions of many procedures, numerous pictures, diagrams, and tables of R_F values. A bibliography and author and subject indexes are provided. Well realized is the authors' hope "that sufficient detailed information is provided, in the majority of instances, so that the reader will be able to apply the technique of paper chromatography to his particular problem without recourse to an extensive search of the literature."

As the authors of these two books have made rather different approaches to the subject, a somewhat complementary relationship is observable. There is further reason to consult them both, since the eclectic bibliographies show numerous differences. For references after 1951 in this rapidly burgeoning field, other sources of information must be sought.

RALPH C. CORLEY

Department of Chemistry, Purdue University

Rocks for Chemists. S. James Shand. New York: Pitman, 1952. 146 pp. Illus. \$4.50.

The author has described this book as "An introduction to petrology for chemists and students of chemistry." This has been well achieved by careful arrangement and lucid explanation of the subject matter. Beginning with a brief historical introduction, he goes on to outline the characteristics of crystalline and sedimentary rocks as seen in the field and points out the differences in texture between plutonic and volcanic rocks. A list and explanation of the terms used to describe the texture of rocks are given.

A chemical system of rock classification is proposed,

based on the presence or absence of free SiO_2 (quartz, tridymite, or highly siliceous glass). A rock containing free SiO_2 plus minerals that can exist in equilibrium with free SiO_2 is termed "oversaturated." One that has no free SiO_2 but only minerals that can coexist in equilibrium with free SiO_2 is called "saturated." He terms "unsaturated" those rocks that contain no free SiO_2 but do contain minerals that could not coexist in equilibrium with SiO_2 , such as nepheline, sodalite, leucite, corundum, etc. Another method of chemical classification of rocks is described in which the molecular ratio of Al_2O_3 to $\text{Na}_2\text{O} + \text{K}_2\text{O} + \text{CaO}$ is the criterion. If the ratio is greater than 1:1 the rock is termed "peraluminous;" if the ratio is not greater than 1:1 but exceeds the ratio of Al_2O_3 to $\text{Na}_2\text{O} + \text{K}_2\text{O}$, the rock is called "metaluminous." Where all the Al_2O_3 is contained in feldspars or feldspathoids, and a nonaluminous mineral such as olivine is present, the rock is designated "subaluminous." The steps in the calculation of the norm of a rock are briefly outlined.

The chapters on the systems plagioclase-pyroxene and olivine-pyroxene-plagioclase draw heavily on the published work of Bowen and Schairer, as does the chapter on feldspathoidal rocks. The chemical and mineralogical composition of sedimentary rocks is discussed. A comparison of alteration that is due to weathering with that due to hydrothermal action is made.

The physical and chemical processes of metamorphism are outlined in the last two chapters, and the importance of H_2O in metasomatic transformations is emphasized.

Preceding the index, under "Suggestions for Reading," a bibliography of geochemistry, containing 40 titles, is listed. Following the index are 32 excellent plates of photomicrographs of thin sections, photographs of polished sections, and views of geological formations.

JOSEPH J. FAHEY

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Gmelins Handbuch der anorganischen Chemie. 8th ed. Edited by Gmelin-Institut für anorganische Chemie und Grenzgebiete. *Sauerstoff*, System No. 3, Part 2: *Vorkommen-Technologie*, 1952. 218 pp. \$15.48. *Arsen*, No. 17, 1952. 475 pp. \$33.33. *Magnesium*, No. 27, Part A-4: *Legierungen von Magnesium mit Zink bis Rhenium; Oberflächenbehandlung*, 1952. 336 pp. \$23.81. *Titan*, No. 41, 1951. 481 pp. \$27.20. Verlag Chemie, GmbH, Weinheim/Bergstr., Germany. U. S. distrib.: Edwards Brothers, Ann Arbor, Mich.

These four volumes represent a valuable addition to Gmelin and a satisfying advance toward completion of the eighth edition. With the continued growth of the chemical literature, the research chemist is becoming increasingly dependent upon reference works such as Gmelin and Beilstein. A literature search through the abstract journals upon any specific topic is becoming prohibitively costly in terms of require-

ments in time. Reference works such as these greatly facilitate such searches. Chemists everywhere would be profoundly grateful if a sharp reduction in the time lag between the appearance of a paper in the literature and its incorporation in the reference works were possible.

Sauerstoff treats the distribution of oxygen, ozone, and water. The geochemistry of the hydrosphere is reviewed. The technology of oxygen, ozone, and hydrogen peroxide manufacture is covered.

The volume on arsenic represents a complete treatment of the geology, chemistry, physics, and industrial utilization of arsenic and its compounds. As mentioned above, the regular appearance of such volumes covering the chemistry of each of the elements, with a delay of no more than five years, would tremendously simplify the time requirements for literature searches.

The magnesium volume deals with binary and ternary alloys of magnesium and is, therefore, of primary interest to metallurgists and engineers.

Titan is a comprehensive treatment of the chemistry of titanium and its compounds. The volume is particularly timely in view of the present active interest in titanium metal. It is of particular interest to American chemists to note that this volume is dedicated to Roger Adams for his assistance in the rehabilitation of the Gmelin Institute following World War II.

These volumes maintain the usual high standards of clarity, conciseness, and organization of previous volumes in the Gmelin series. They represent an essential part of the literature of inorganic chemistry and should be a required item in every library used by research chemists.

HERBERT C. BROWN

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The Biological Sciences

Evolution in the Genus Drosophila. J. T. Patterson and W. S. Stone. New York: Macmillan, 1952. 610 pp. Illus. \$8.50.

An appreciation of the importance of this book is best gained by a brief survey of its historical background. During the early decades of the twentieth century little interest was taken by biologists in the subject of evolution as a field for active study and research. However, the new science of genetics was rapidly advancing, particularly owing to the brilliant researches of the Morgan school on the genetics of *Drosophila*.

By 1927 H. J. Muller, then at the University of Texas, had demonstrated the artificial induction of mutations in *Drosophila* by x-rays. A few years later, in the same laboratory, T. S. Painter, ably assisted by W. S. Stone, had demonstrated the significance of the constant banded pattern of the giant salivary gland chromosomes of *Drosophila*—not merely an advance

in cytology, but rather an entirely new level of cytology. In the meantime A. H. Sturtevant had combined taxonomic and genetic studies of *Drosophila* species and demonstrated hybridization between two species of the genus. Genetic analysis of *Drosophila* populations by Sturtevant and Th. Dobzhansky, N. P. Dubinin and colleagues, and others had thoroughly disproved the old bromide of the irrelevance of "bottle mutations" in evolution studies.

About 1938 J. T. Patterson and his colleagues at the University of Texas, aided by a generous grant from the Rockefeller Foundation, turned their attention to the problems of speciation and evolution in the genus *Drosophila*. With a truck fully equipped for collecting and field studies, workers from this laboratory made many extensive collecting trips throughout the U. S. and Mexico, and brought back alive for taxonomic, genetic, and cytological study approximately 100 new species, and many previously described forms. A series of University of Texas bulletins, totaling over 1600 pages, many hundreds of tables, figures, and diagrams, with detailed figures of internal anatomy, colored plates of over 70 forms, and many salivary and metaphase chromosome plates, has been issued at intervals, reporting the progress of these researches on speciation and evolution in the genus.

This work, published and in progress, naturally forms much of the basic material for *Evolution in the Genus Drosophila*. The book, however, is a comprehensive review of the work on taxonomy, geographical distribution, hybridization, genetics, and cytology of the genus in many laboratories in this and other countries, with a synthesis and interpretation of this material as it bears on the subject of evolution. It is this reviewer's opinion that the authors have done an unusually thorough and unbiased job of evaluating the contributions of other laboratories, and of interpreting the findings of the many investigators in this field. In the bibliography of some 700 references, perhaps half the items have been published since 1938. The early literature has not been neglected, however, and those who have provided the background for the recent rapid advances have been given due credit.

A résumé of the taxonomy of the genus, with descriptions of 7 subgenera and 29 species groups to which 267 species are assigned, is followed by a section dealing with geographical distribution, possible phylogenetic relationships, and what is known of the ecology. In the chapter on chromosome evolution, figures of the metaphase chromosomes of over 120 species are shown, with illustrations and interpretations of many changes in chromosome configuration through translocations, fusions, and inversions.

Other sections deal with salivary chromosomes, gene variation, selection, and genic balance, and isolating mechanisms. Over 100 cases of hybridization, showing differing degrees of evolutionary divergence, are described. One chapter is devoted to the remarkable story of evolution in the *virilis* group, as revealed through a study of the geographical distribution, hybridization,

sexual isolating mechanisms, and cytology of the 10 species and subspecies of this group. A final chapter on comparisons and conclusions relates the *Drosophila* studies to those on other organisms and to the broad field of evolution.

The publishers are to be commended for and complimented on the excellent reproduction of the many figures, and the almost total absence of typographical errors. This book will serve as a necessary handbook for research students, a mine of material for the writers of texts and other treatises, and a fascinating and authoritative exposition of the *Drosophila* story for all those interested in animal evolution.

WARREN P. SPENCER

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The Tropical Rain Forest: An Ecological Study.

P. W. Richards. New York: Cambridge Univ. Press, 1952. 450 pp. Illus. \$12.50.

This comprehensive and scholarly study of the tropical rain forest has so great a variety of data that this review can do little more than whet the appetite of the botanist, plant ecologist, forester, geographer, tropical agriculturalist, and others. So far as this reviewer is aware, this volume is the first major contribution dealing with the botany and plant ecology of the rainy tropical forest since Schimper's *Plant Geography* (1898, 1903, 1935).

The rainy tropics are demanding increasing attention over much of the world. Our general ignorance of these areas has been colossal and inexcusable. Even the average educated person does not realize that this nonseasonal tropical rain forest comprises about half the world's forest area. Nor does he know that man has been so destructive of this forest that, unless he alters his ways very soon, he may destroy it within a lifetime. Most people believe that the tropical rain forest remains in its pristine condition and that as yet man has scarcely made a dent in it.

A few high spots that should prove interesting to anyone with intellectual curiosity about the wet tropics and their flora are:

1. There is no pure stand: if the traveler notices a particular species of tree and wishes to find more like it, he may often turn his eyes in vain in every direction.
2. The forest is often two- and three-storied.
3. It is not difficult to penetrate the forest except on riverbanks or clearings. (A photograph is used to show this.) It is the slippery clay soil and the abundance of fallen logs and branches which make progress in the forest slow and laborious, rather than the thickness of the vegetation.
4. The structure of the forest on a riverbank is always different from that in the interior.
5. When trees are being felled, a large tree is often so strongly bound to its neighbors by lianas that even when cut at the base it will not fall.
6. The common idea that a thick layer of litter is characteristic of the rainy tropical forest is completely fallacious.
7. The tropical rain forest is not important as a source of timber despite the fact that it is the only

source of such woods as greenheart and several kinds of mahogany. This results from the fact that the greater part of the world demand for timber is for coniferous softwoods, which are usually absent in the tropical lowland.

8. In all probability the great "open" savannas of the world (e.g., the llanos of northern South America) represent biotic climaxes that are usually due to fire or edaphic climaxes caused by soil conditions unfavorable to trees. It is doubtful whether any tropical grassland is a true climatic climax.

This reviewer strongly recommends the book to all those with a scientific or economic interest in the rainy tropics. There are some omissions, to be sure; probably the most serious was the author's decision to skip over man's economic relation to the tropical rain forest. (He did not live up to his decision altogether, however.)

The author admits shortcomings, admits even that the science of ecology is not yet ripe for a rigid theoretical framework. Although criticizing the broad generalizations of Schimper, he states that, in the present state of our knowledge, it is impossible to put in their place any clear or definite conclusions.

The Tropical Rain Forest contains many photographs, which are not only clearly reproduced but tell their story well. There are also several good maps, a large number of drawings, charts, and tables. There are an excellent bibliography in many languages, an index to plant names, and a general index.

C. LANGDON WHITE

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Theoretische Biologie: Stoffwechsel, Wachstum,

Vol. II. 2nd ed. Ludwig von Bertalanffy. Bern: A. Francke, 1951. 418 pp. 43.50 Sw. fr.; bound 47.50 Sw. fr.

Biologists generally will welcome this new edition of the volume on physiology and growth in von Bertalanffy's well-known treatise. Like the first edition, this one covers a truly prodigious amount of material. It can be compared only with such works as Needham's *Biochemistry and Morphogenesis*, or D'Arcy Thompson's *On Growth and Form*.

The section on physiology, which constitutes the first two thirds of the book, is heavily, and quite properly, biochemical in emphasis—although there are long chapters on physical and physico-chemical aspects of living things. The text has been brought up to date by the inclusion of new results in many different areas. The Cori enzymes, phosphorylation, and the Krebs cycle are discussed, as is the work of Caspersson and Brachet on nucleic acids, of Darlington, Wright, and others on plasmagenes, of Pauling on antibody formation, of Horowitz, Beadle, and others on gene-controlled biochemical syntheses in *Neurospora*, of numerous investigators on the mechanism of photosynthesis, and the clinical importance of ACTH—the list could be extended indefinitely.

As is inevitable in a single volume covering such a

vast field and written by a single man, it is not difficult to find omissions. For example, the work of Pollister and Mirsky appeared to be missing from the discussion of protein synthesis and chromosome constitution, and the work of Evans from the discussion of pituitary hormones and growth. The present reviewer would like to have seen a fuller discussion of the relationship of photosynthesis to respiration which, as it stands, is little more than a footnote. But the section as a whole is such a rich mine of facts and ideas that it would be an ungenerous reader indeed who was not glad to accept von Bertalanffy's overly modest apology, which takes the form of a quotation from Aldous Huxley: "The subject is vast and complex; this volume is short and the knowledge and abilities of the author narrowly limited."

The final third of the book, on growth, is primarily mathematical in approach, in the manner made familiar by Thompson, J. Huxley, and von Bertalanffy himself. It presents a masterly survey of the work of the thirties and forties that analyzed growth phenomena in individuals and in populations by exponential equations, calculus, and other mathematical devices.

No doubt some readers would have preferred a more extended treatment of the author's views on the problem of "organic wholeness." Presumably, such a treatment will be found in Volume III, on "Dynamic Morphology," which von Bertalanffy promises. Meanwhile, we shall have to be satisfied with the recently translated and extremely interesting *Problems of Life*, in which von Bertalanffy deals with this topic in non-technical language.

GAIRDNER B. MOMENT

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The Epigenetics of Birds. C. H. Waddington. New York: Cambridge Univ. Press, 1952. 272 pp. Illus. \$7.00.

His central purpose in writing *The Epigenetics of Birds*, Waddington tells us, "has been to review the literature, mainly dating from the 1930's, which deals with the primary stages of morphogenesis [in the chick]." In fulfilling this purpose the author has assembled an invaluable compendium of experimental work on, roughly, the first 5 days of chick development. The significance of each case treated is extracted in a manner almost deceptively succinct and convincing, the effect being heightened by the ease and grace of Waddington's literary style.

The book opens with a brief consideration of technical methods and then surveys the earliest stages in the differentiation of the blastoderm. This chapter provides a thorough review of the newer work of Peter, Pasteels, Spratt, and others, although it is somewhat overweighted with the minutiae of map-making for the blastoderm. The third chapter is a stimulating if inconclusive attempt to interpret the derivation of the large-yolked sauropsidan egg from the small-yolked amphibian egg by comparing the distribution of the fu-

ture germ layers in the blastula stages of the two. Chapter 4, on endoderm formation, establishes the "gradient-field" character of the hypoblast.

Chapters 5 and 6 deal with the attempts that have been made to determine the potentialities of portions of the blastoderm, including the primitive streak, by operative intervention. Here, Waddington addresses himself with marked success to one of the major aims of the book—that of reviewing the results of avian experimental embryology in relation to those of amphibian embryology. Although he brings out the close resemblances of epigenetic procedure in these two classes, he also considers carefully the technical limitations that have made the results of chick embryology somewhat the less conclusive.

The seventh chapter, dealing with biochemical studies of early stages, reveals principally the poverty of both ideas and results in this fundamental field. Chapter 8 is devoted to a rather superficial study of mosaic differentiation of organ systems in later stages, and Chapter 9 provides a useful review of plumage development. Chapter 10 summarizes the studies that have been made on genetic defects and their phenocopies in chick embryos; although thorough on the factual side, the treatment contributes less to our understanding of this intricate subject than Waddington's authority in the field of genetics might lead us to anticipate.

Critical discussion of the empirical data that comprise the bulk of the book is largely postponed to the last chapter, which, it must be regretfully admitted, is not the most successful part of the work. The chapter is brief (only 15 out of 239 text pages) and says little more than is revealed on the surface of the data themselves. The insights we might have expected are not here, nor is there any really cogent attempt to orient the results of experimental studies in avian epigenesis in relation to the frontiers of modern physiological research. On the other hand, the book is certainly not uncritical; and the emphasis on fact and analysis, rather than on theory and synthesis, has had the good effect of keeping the whole within conveniently small compass.

A minor and, one hopes, unintentional defect of the book is a disquieting nationalism. All embryology seems to be divided into three parts: a German "school" devoted to amphibian embryology, and rival English (largely Waddington) and American "schools" of chick embryology. This curious view of science leads to results both amusing and disturbing. So "Willier and his pupils" are made to share the blame for the limitations of Lillie's embryonic segregation hypothesis because they are Americans, whereas R. G. Harrison goes unmentioned, apparently because he is not German. The various French-speaking authors whose works are liberally dealt with are, perhaps from lack of common nationality, allowed to pass unschooled.

Whatever flaws one might find in the work, however, do not alter the fact that *The Epigenetics of Birds* represents a major contribution to the literature of experimental embryology. Without doubt it will become

a standard reference work for every practicing embryologist.

FLORENCE MOOG

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Mammals of Utah: Taxonomy and Distribution.

Univ. Kansas Pubs., Museum of Natural History, Vol. 6. Stephen D. Durrant. Lawrence: Univ. Kansas, 1952. 549 pp. Illus.

Mammals of Kansas. Univ. Kansas Pubs., Museum of Natural History, Vol. 7, No. 1. E. Lendell Cockrum. Lawrence: Univ. Kansas, 1952. 303 pp. Illus.

Utah and Kansas are now added to the scant dozen of the states having adequate systematic accounts of their respective mammalian faunas. These two books have much in common besides sponsoring institution, format, and general plan of treatment. Each author is a field-and-museum naturalist who is thoroughly familiar, not only with the kinds of mammals and how they differ from one another in structure and distribution, but also with the physiographic and climatic conditions of their environment in the specific area treated. Such a combination of knowledge is indispensable if zoogeographical problems are to be intelligently handled. Moreover, these authors have personally inspected many thousands of specimens and have studied the original field notes of numerous colleagues. Each of the resulting volumes, therefore, contains much new information and gives a well-balanced picture of the current state of knowledge in the field it covers.

The primary approach is systematic. Keys, descriptive statements, measurements, and distribution maps make the reports usable as manuals for identification. Comparatively little of the text will be attractive to the popular reader, but these books are certain to be used for background data and orientation by future writers of popular natural history. For serious students of mammalian systematics they will become standard works of reference.

The diverse and varied mammalian fauna of Utah is treated by Durrant only in regard to taxonomy and distribution. Analysis of intraspecific geographic variation occupies most of the author's attention. The accounts of rodents, which include about 52% of the total number of species in the state, occupy about 69% of the text. Carnivores, in contrast, are briefly treated; grizzly bears are disposed of in less than a page, whereas one species of rodent, the pocket gopher *Thomomys bottae*, receives 53 pages. A carefully thought-out terminal section discusses the general and theoretical matters of faunal areas and the apparent effects on speciation of physiographic features. The part played by Pleistocene Lake Bonneville in shaping the modern fauna is shown to be especially important.

For Kansas, with fewer kinds of mammals and less contrasting physiography, Cockrum has provided more inclusive species accounts. General statements on habitats, breeding, and economic importance are added

to the systematic data. Most of the natural history material is compiled from the publications of other authors, and it is disappointing to find so little of the original observations that Cockrum and his co-workers have had opportunity to make in recent years.

A regional account of the fauna provides the ideal medium in which a mammalogist may combine his own observations with pre-existing knowledge to produce a comprehensive statement concerning the mammals of his favorite area. Such an account also provides the most readily accessible source of information for the student or researcher. It is to be hoped that books with titles beginning "Mammals of . . ." will continue to appear.

DAVID H. JOHNSON

Division of Mammals

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Soil Microbiology. Selman A. Waksman. New York: Wiley; London: Chapman & Hall, 1952. 356 pp. Illus. \$6.00.

Following an excellent historical introduction, this volume deals with the kinds and numbers of soil microorganisms and the methods for studying them. The important groups are briefly described, with a chapter devoted to their decomposition processes. The chemical equations for carbohydrate decomposition will probably mislead the student into regarding them as reactions and products which occur in the soil, whereas they are not actually representative of the metabolism of any known organism or group of organisms.

The chapter on humus is particularly good and is followed by a discussion of the various methods for determining the rate of decomposition of humus and other organic matter in the soil. Nitrogen transformations are introduced by a discussion of the decomposition of protein and other nitrogenous compounds and the resultant formation of ammonia, followed by a consideration of nitrification and denitrification. Separate chapters are devoted to symbiotic and non-symbiotic nitrogen fixation, and a greater importance of symbiotic fixation in maintaining soil nitrogen levels is emphasized. Another chapter describes the transformations of sulfur, phosphorus, and other elements.

The interrelations of soil organisms are considered in a chapter on the relations between soil microorganisms and higher plants, a second one on "associative" and antagonistic effects between soil microorganisms, and a third on plant pathogens in which their control by nonpathogenic microorganisms, as well as by other treatments, is discussed. The final three chapters deal with (1) stable manures, composts, and green manures, (2) microorganisms and soil fertility, and (3) recent developments.

The book is designed as a short introductory text and is an excellent account for anyone desiring an acquaintance with the microbial activities in the soil.

The problems in soil microbiology, as well as the chief contributions, are emphasized. Most of the literature quoted is quite old. In some instances, this is desirable, because it gives credit to the investigators who discovered important processes and directs the student's attention to some of the classics. In other cases, recent significant work is not mentioned.

From the standpoint of the microbiologist, relatively little attention is paid to the nature and characteristics of the organism concerned with the various soil processes, but the significance of their activities in soil fertility receives excellent treatment. The author's long interest and activity in soil microbiology are reflected in the pages of the book.

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The Earth Sciences

Traité de Paléontologie. Vol. I, Les Stades Inférieurs d'Organisation du Règne Animal—Protistes, Spongiaires, Coelentérés, Bryozoaires; Vol. II, Problèmes d'Adaptation et de Phylogénèse—Brachiopodes, Cétognathes, Annélides, Gépériens, Mollusques. Jean Piveteau, Ed. Paris: Masson, 1952. Vol. I: 782 pp. Illus. 8300 fr.; clothbound, 8900 fr. Vol. II: 790 pp. Illus. 9000 fr.; clothbound, 9700 fr.

These are the first two of seven volumes covering the field of fossil animals. ("Traité de Paléozoologie" would be a better title, as fossil plants are excluded.) The major phyla of invertebrates, except the arthropods and echinoderms, and some minor phyla are treated in these volumes, to which 16 collaborators, all French with the exception of one Belgian, have contributed. The remaining invertebrates are to be dealt with in the third volume, and four volumes are to be devoted to the vertebrates.

Volume I starts with a series of introductory chapters on the history of paleontology; fossilization; principles of systematics; paleontology, chronology, and ecology (very brief); and on phylogenesis of the animal kingdom.

The systematic part gets off to a good start with the most complete treatment of fossil protists in any one book. Of the 13 groups of protists in this volume, discussion of all except the foraminifera is the work of Georges Deflandre, who is responsible for much of the modern literature on the 12 groups.

Jacques Sigal wrote the useful chapter on the foraminifera, except the part on the fusulinids, which is by Raymond Ciry. Classification of the foraminifera is still in a state of flux. Sigal's classification is most like Glassner's 1945 arrangement, although Sigal recognizes 61 families and Glassner 33. The chapter closes with a lengthy and valuable account of the ecology and paleoecology of foraminifera.

Sponges are rather summarily treated by Léon

Moret. The archaeocyathids are given only four pages. The coelenterates are taken up piecemeal, without any synthesis of the classification of the phylum. The important rock-forming stromatoporiids are inadequately treated in the chapter on hydrozoans.

The corals take up a third of Volume I. The Paleozoic "madreporarians" (tetracorals, tabulates, and heliolitids) are admirably handled by Marius Leconte. His adoption of English terminology for the vegetative forms of tetracorals, because it is the best available and for the sake of uniformity, is a significant indication of the breadth of treatment. He proposes a new superfamily classification of the tetracorals that emphasizes septal structure. At the outset, however, he serves warning of the possibly unevaluated hazards of deceptive homeomorphy. He also recognizes the difficulty of establishing phylogenetic relations in the tabulate corals.

The chapter on post-Paleozoic "madreporarians" (hexacorals or scleractinians of other authors), by J. Alloiteau, evidently aims at complete generic coverage, unlike other parts of the volume. Presumably, it is based on a thesis presented by Alloiteau at the Sorbonne in 1952 (p. 682, item 1). Although still unpublished, the thesis may be assumed to be the source for the numerous new generic names cited as "Alloiteau, 1951." Some of these are based on new, undescribed, and unfigured species, also cited as "Alloiteau, 1951." Page 542 promises a later discussion of the modes of colony formation, but through some inadvertence that important subject is not again mentioned. In the systematic part terms are used without any explanation to describe modes of colony formation. Alloiteau's classification is a modification of that proposed by Vaughan and Wells in 1943. Three new suborders and many new families and subfamilies, some of which are of faulty orthography, are added. The classification is based primarily on the concept that the surface aspect of the septa reveals the trabecular structure.

Volume I closes with a chapter on bryozoa, by Emile Buge, that does not come up to the standard set for the other phyla. The classification is essentially an abbreviated version of Bassler's 1934 classification in *Fossilium Catalogus*. The general discussion of morphology is brief, and the relatively few genera in the systematic part are too briefly diagnosed. A closing résumé of the ecology of bryozoa and their association with other animals is informative and stimulating.

Aside from 42 pages on worms and wormlike animals, Volume II is taken up by the brachiopods and mollusks. Many paleontologists and zoologists would disagree with class rank for bryozoa, brachiopods, annelids, and other wormlike animals under a phylum Vermidians ("embranchement des vermidians").

The lengthy chapter on brachiopods is a tour de force, as it appears to be Jean Roger's first contribution to brachiopod literature. He leaned on Schuchert's and LeVené's 1929 comprehensive listing of genera and higher categories in *Fossilium Catalogus*

and on literature since 1940. Incomplete coverage of the literature of the 1930s results in omission of the results of some important monographs. The classification adopted is an unsatisfactory compromise between Cooper's 1944 avowedly nonphylogenetic pigeonhole arrangement, based on shell structure, and Beecher's classification as modified by Schuchert. Generic coverage is uneven. It is practically complete for the inarticulates and about 75 per cent complete for the articulates.

Colette Dechaseaux adopts a slight modification of Douville's 1912 classification of the lamellibranchs. Dall's 1895 classification, popularized in the Eastman-Zittel textbook and widely used in America, is barely mentioned in the historical summary of classifications. Generic coverage is poor in many of the families. The account of shell structure by Gabriel Lucas, dealing with a little-used taxonomic tool, and the systematic part covering the rudists, are certain to be widely read and used. The discussion of the morphology of rudists and the generic diagnoses are much better than for other lamellibranchs.

The chapter on gastropods, by Geneviève and Henri Termier, is not likely to be useful to a beginner or a specialist. The classification is fantastic.

Volume II closes with three chapters on the cephalopods that reach a high standard. The chapters on nautiloids and ammonoids, except the elymenids and goniatites (by Gaston Delépine), were written by Eliane Basse (Eliane de la Goublaye, Vicomtesse de Ménorval), and the chapter on dibranchs by Jean Roger. These chapters bring together better than anywhere else a useful summary of modern views. The classification of the nautiloids rests heavily on Flower and Kummel (1950), and the classification of the ammonoids on the views of Arkell, Spath, and C. W. Wright. The ammonoid arrangement represents a clean break with Douville's classification (unpublished, but distributed to his students at the Ecole des Mines), which has dominated French work since the turn of the century.

The textbooks and treatises on which several generations of students in Western Europe and America have grown up are badly outdated, leaving a vacuum that is being filled by Piveteau's *Traité*. The format of the *Traité* is good. With few exceptions, the line cuts and the halftone and collotype plates are satisfactory; in fact, many of the line cuts throughout the volumes and the halftones of rudists are superb. No enlargement, however, is shown for the illustrations of protists (other than foraminifera), and none of the illustrations for gastropods, nautiloids, and ammonoids has any indication of enlargement or reduction. Uneven treatment is to be expected with so many collaborators, as is the emphasis on Western European genera. The uneven treatment is well illustrated by the bibliographies. Some represent open doors to the essential literature, others are avowedly limited to recent contributions, and others are inadequate from any point of view. Some trivial items have been admitted

in two bibliographies. All the bibliographies have one defect—there is no indication whether a publication consists of one page or of hundreds. The one real drawback, however, is that not many students will be able to afford these volumes.

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¹ I am indebted to the following paleontologists for advice: P. E. Cloud, Jr., (Paleozoic corals, brachiopods); H. M. Duncan (bryozoa); A. R. Loeblich, Jr., (foraminifera); J. B. Reeside, Jr. (cephalopods); J. W. Wells (sponges, coelenterates in general, and scleractinians in particular).

Europe. 2nd ed. Samuel Van Valkenburg and Colbert C. Held. New York: Wiley; London: Chapman & Hall, 1952. 826 pp. Illus. \$7.50.

This is a book on Europe which will not only serve as a textbook, but can be recommended to anyone interested in world problems. This is because the authors are conscious throughout of the many problems that beset the nations of Europe today. Some of these are basically geographic—arising from the location of Europe as a whole or of its individual countries, from natural endowment or climatic conditions. Other problems can be better understood if one knows the geographical setting in which they arise.

There is a certain emphasis on climatic influences and national heritage, an emphasis retained from the first edition and its coauthor, the late Ellsworth Huntington. Indeed, a few chapters on physical geography, and that on the march of civilization, have been preserved as a tribute to the memory of this original thinker. Nonetheless, others of Huntington's contributions have been omitted, such as the chapter on races. The discussion of ethnographic features was reduced sharply, although still retaining much of the original contribution. Thus the maps of cephalic index complexion and stature are still there, although the text which they illustrated is gone. Most of the chapters have been completely rewritten, however. No attempt has been made simply to bring obsolete chapters up to date. Europe of 1952 is no longer the Europe of 1935! Quite rightly the authors treat human geography as an ever-changing aspect of the earth's surface as much as they stress the lasting features of its physical nature.

An independent approach, unfettered by formal adherence to rules, is also shown by the way individual countries are treated. Each country is discussed according to its particular character, following an individual outline, and so a vivid treatment of each country is assured. On the other hand, the unity of Europe is never lost from sight. Possibly, it is easier for an observer from another continent to realize this essential unity than for a European to do so. Consistently, throughout the book, much stress is laid on the difference between the highly developed northwestern Europe A and the backward southern and eastern Europe C—a concept introduced by the French economist Delaisi. This division does not follow political boundaries. For example, Italy, with its northern

provinces, belongs to Europe A, southern Italy to Europe C, with a transitional Europe B zone in between.

The text is illustrated by numerous and, in the great majority, very good maps. Many are entirely new, others have been completely redrawn. The book is also illustrated with a relatively small number of beautiful photographs. However, their small number, one or two for each country (only Italy and the Netherlands rate three), raises a difficult problem. The authors apparently felt that the omission of photographs from the first edition was not satisfactory. On the other hand, the presentation of all or of the most important distinctive feature by photograph, as was done in such a fine way by maps, would necessitate an almost prohibitive price for the book. Therefrom, apparently, resulted a not quite satisfactory compromise of publishing only a few characteristic photographs. A good bibliography concludes this useful and well-written book.

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Principles of Invertebrate Paleontology. 2nd ed. of *Invertebrate Paleontology*. Robert R. Shrock and William H. Twenhofel. New York-London: McGraw-Hill, 1953. 816 pp. Illus. \$12.00.

Invertebrate Fossils. Raymond C. Moore, Cecil G. Lalicker, and Alfred G. Fischer. New York-London: McGraw-Hill, 1952. 766 pp. Illus. \$12.00.

Fourteen years have elapsed since an American textbook in invertebrate paleontology last appeared. They were years marked by continued expansion in petroleum exploration and consequent demand for trained paleontologists; years marked also by important systematic discoveries calling for revision of teaching materials.

These two books are similar in scope and plan. Both are aimed at the student-specialist, so we still lack an American book suitable for a student of more general interests. Both use a stereotyped approach, with each phylum or lesser group discussed in terms of the morphology of living representatives (if any), geological distribution, ecological significance, and classification. The differences between the two books are minor, but real. Shrock and Twenhofel's material seems to be consistently better integrated and more effectively presented for student use. Some of their chapters—e.g., those on the insects and the conodonts—are excellent, whereas those by Moore *et al.* are merely adequate.

In such books the quality of the illustrations is of great importance. Except for one page of halftones, which reproduced badly, Moore *et al.* have chosen to use bold ink drawings which, in some cases, especially in the section on echinoderms, are quite unattractive. In dealing with such forms as smooth brachiopods, ammonites, and ostracods, the problem of shading to suggest contour while still conveying the gross appearance of the fossil has not been solved. Shrock and Twenhofel

use a variety of illustrative techniques. There are many photographs, generally very well reproduced; and where drawings are used, the line is thin enough to make a pleasing page. Bibliographies in both books are adequate, with Moore *et al.* showing a tendency to load the lists with relatively trivial American titles and to omit important European studies, which Shrock and Twenhofel include. It is odd, for instance, to see such detailed work as Moore's echinoderms with no mention at all of Whitehouse's basic research on the Haplozoa.

After studying these books carefully and trying parts of them on students, I feel that, although each is better than any previous American text, Shrock and Twenhofel is definitely to be preferred for both teaching and reference.

I confess to ignorance of economics, but the reasoning that led McGraw-Hill to bring out these two books in a specialized field in rapid succession baffles me. I am sure the combined sale of the two will be approximately what either of them would have enjoyed by itself. How large that sale will be I do not know, for I feel that, even in these days, \$12.00 is too much to ask a student to pay for a single textbook. McGraw-Hill seem to agree, for I understand that they plan to keep in print the earlier Twenhofel and Shrock *Invertebrate Paleontology*, which is still a good book that is half the size and sells at half the price of the new ones.

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Advances in Geophysics, Vol. 1. H. E. Landsberg, Ed. New York: Academic Press, 1952. 362 pp. Illus. \$7.80.

When the term "geophysics" was mentioned a few years ago the listener might picture a few hardy "doodlebuggers" stretching wires over the countryside, attaching instruments, and then drilling shot holes in which a charge of dynamite might be exploded. Turkey growers might claim that the explosion ruined their hatching eggs, but the petroleum geologist knew it would demonstrate the best location to drill for oil. Those better informed might also have included students of earthquakes in the school of geophysicists, but geophysics went no further in the eyes of most.

The science of geophysics is the application of physical principles and measurements to a study of the earth, and this includes all branches of physics to all parts of the earth. When the reader learns that the present book does not even list "earthquakes," "seismograph," or "petroleum" in its index, he realizes that the branches of geophysics must be many.

Dr. Landsberg has made no small contribution to the scientist's library by assembling these papers. The articles, eight in all, include such subjects as higher atmospheres, shoreline hydrography, gravity, aeromagnetism, and the processing and statistical studies

of geophysical data. The articles have been clearly presented, and in most cases a table of symbols and a bibliography are given. The figures employed are clear and well reproduced, although one might desire more illustrations of equipment. The appearance of the book is pleasing, and the choice of type and the general make-up are well done.

The first paper, on the "Automatic Processing of Geophysical Data," might not seem important to some, but in research bureaus where a mass of data is being accumulated each day a process of this sort is necessary. The methods employed by geophysical departments of the various petroleum companies could have been included here. The article on statistical methods is also important. Although these techniques are being used more generally in the field of geophysics, they should be employed with care. As the literature has shown in the past, too many untrained statisticians have brought forth weird theories by the improper use of accurate data.

The studies on the atmosphere and estuarine hydrography are clear and comprehensive. Dr. Woolard's paper on gravity is excellent, both for interest and for coverage, as is Balsley's article on aeromagnetism.

To the expert or advanced student the book provides little that is new in that person's respective field. The papers are too brief to contain the fine points specialists would be seeking. The book has fulfilled the end of explaining some of the branches of geophysics to the uninitiated, however, and Dr. Landsberg is to be commended for this. It is hoped that future volumes will prove as helpful.

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Volumetric and Phase Behavior of Oil Field Hydrocarbon Systems. M. B. Standing. New York: Reinhold, 1952. 123 pp. illus. \$10.00.

This book will be a welcome addition to the tools available to the petroleum engineer, the scientist, and the field man associated with measuring and interpreting the behavior of oil field hydrocarbon systems. Available but widely scattered data have been compiled with an appreciable amount of original material into a working manual for reservoir hydrocarbon calculations. The book is liberally interspersed with usable graphs and many sample calculations in sufficient detail for any good student to follow with ease. It should be particularly helpful in training laboratory technicians, and the chapter on "Sampling Methods and Apparatus" can be profitably read by many a field engineer.

Oil field hydrocarbon systems are treated in three classes: gases, condensate systems, and dissolved gas systems, with supporting chapters on general phase behavior of hydrocarbons, sampling methods, and reservoir material-balance calculations. This latter chapter will be valuable to the newcomer for its clear dis-

cussion and examples of the use of laboratory PVT data in reservoir calculations based on the Schilthuis equation and others. Three valuable charts, in a pocket on the inside back cover, enable approximation of formation volumes of both condensate and dissolved gas systems, and bubble-point pressures of the latter, from readily available field data such as gas-oil ratio, gas and tank oil gravities, and reservoir temperatures and pressures. These empirical correlation charts will save the practicing reservoir engineer much time in approximating and checking calculations on the physical properties of reservoir fluids.

The book is not for the advanced scientist looking for extensions and innovations of theoretical treatment. It does, however, do an excellent job of definition and explanation of the necessary terminology and theory for the working calculations. The book is aimed at the practicing reservoir engineer and technician and will be greatly appreciated by them.

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Psychology

Psychoanalysis as Science. Ernest R. Hilgard, Lawrence S. Kubie, and E. Pumpian-Mindlin. Stanford, Calif.: Stanford Univ. Press; London: Geoffrey Cumberlege, Oxford Univ. Press, 1952. 174 pp. \$4.25.

This volume presents the Hixon Lectures on "The Scientific Status of Psychoanalysis," delivered at the California Institute of Technology. The contents concern the frequent question of whether the discipline of psychoanalysis is a science, and how its methods, theories, and findings may be subjected to scientific investigation and validation.

Dr. Hilgard, chairman of the Department of Psychology at Stanford, opens the series with a discussion of "Experimental Approaches to Psychoanalysis." He presents a thoughtful selection of the extensive experimental work done on psychodynamics with animals, children, and adults. The evidence tends to confirm certain psychoanalytic concepts and theories. As Hilgard carefully points out, however, to show that theoretical relationships proposed by psychoanalysts are plausible does not solve the problem of how the relationships are caused, or prove that the analytic method is scientific. Experimental studies of psychotherapy of human neurosis and of artificially induced neurosis in humans and animals show satisfactory correspondence with the predictions of psychoanalytic theory. Although these touch only the more superficial problems posed by psychoanalysis, they indicate the way toward more profound critical studies.

"The Position of Psychoanalysis in Relation to the Biological and Social Sciences" is the final lecture, given by Pumpian-Mindlin, chief of the Mental Hygiene Clinic in the Los Angeles Veterans Administra-

tion. This is an interesting historical review of analysis as it developed from a basis of man as a biological phenomenon and as it views man as a product of his social milieu. Because of the complexity of the relationships it attempts to decipher, Mindlin feels that the correlations and hypotheses of psychoanalysis cannot be as precise as those of the more exact sciences.

The real meat of this book is in the lectures on "Problems and Techniques of Psychoanalytic Validation and Progress" given by Kubie, a practicing analyst in New York City and clinical professor of psychiatry at Yale. The reviewer has never seen a more objective, accurate, or penetrating critique of the psychoanalytic method, or a more astute delineation of a problem in dire need of investigation. Kubie boldly and wisely suggests that it is unnecessary to undertake laboratory validation of the observations on the facts of human behavior made by psychoanalysts. These are obvious to any young parent or other observer who is not too reluctant to look human nature in the eye.

The things needing more thorough investigation are: (a) the basic exploratory tool of psychoanalysis, free association, (b) the forces of the transference, and (c) the problem of the validation of interpretations. Greater understanding and increased precision of these basic features of the analytic process are urgently needed. The validation of fundamental psychoanalytic principles of dynamics, genetic theories, personality structure, and causal relationships depends upon the application of techniques of greater quantitative and qualitative precision than are yet available. Kubie outlines an abundant list of problems and tentative approaches to them, hoping the future will bring contributions from allied psychological disciplines and the more exact sciences to clarify and validate the psychoanalytic method.

For psychologists, psychoanalysts, and any others interested in this field, the reading of this book should be most stimulating and rewarding.

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Appraising Personality: The Use of Psychological Tests in the Practice of Medicine. Molly Harrower. New York: Norton, 1952. 197 pp. \$4.00.

This book has as its primary aim the demonstration of the contribution which the test methods of the clinical psychologist can make to the physician's evaluation of both psychologic and somatic disorders. Parts I and II, which deal with clinical psychology in general and with descriptions of specific test procedures such as the Rorschach and the Wechsler-Bellevue scales, take the form of a dialogue between an "inquiring" physician, who takes the tests himself, and a clinical psychologist in private practice. Part III, which consists of case reports, takes the form of exchanges of letters between the physician (now referring patients for examination) and psychologist.

Written in an engaging, rather breezy manner, the presentation is quite effective. One gains a clear impression of what a clinical psychologist is, what he tries to do when he sets up in private practice, what his procedures purport to measure, how he goes about interpreting performance, and how the interpretation helps in the individual case. Although the picture is clear enough, it is not, in the reviewer's opinion, an altogether well-rounded one. As is perhaps natural in a book by a medical psychologist addressed to physicians, the field of clinical psychology is presented as a sort of laboratory adjunct to medicine, providing formal psychodiagnostic services to the physician. This is a formulation which the majority of clinical psychologists would hold to be much too restricted. As conceived by its practitioners, clinical psychology consists not only of formal diagnostic functions (the "method of tests") but also of more comprehensive behavioral evaluation and of techniques for the modification of behavior, such as counseling, re-education, and psychotherapy.

Psychological test methods are introduced by way of the timeworn (and thoroughly unsound) "X-ray analogy," wherein they are conceived as being able somehow to penetrate to the basic personality structure and the fundamental dynamics behind behavior. Although this metaphor may have had some utility in the past, it is surely time that test methods be presented for exactly what they are—namely, measurements of selected behavioral samples with actual or assumed predictive significance. In this regard, it must be said that a good deal of psychodiagnostic work, particularly with projective techniques, rests upon assumptions which either still lack empirical validation or have been demonstrated to be probably invalid. It is in this area of critical evaluation of the procedures themselves that the book shows a decided weakness. Interpretations of details of performance on the Rorschach and drawing tests are rather glibly presented as having a solid foundation in controlled clinical experience which they do not in fact possess. The use in clinical practice of the Szondi test, a procedure that has not met empirical tests of validity, is defended on the ground that "nonetheless, it works and can be extraordinarily helpful at times." But, of course, whether the test "works" is precisely the question which systematic validation study has attempted to answer. Here, an analogy is drawn with the Wassermann test as a procedure, the exact nature of which is unknown, but which nevertheless "works." The analogy is inappropriate. Regardless of what is known or not known about the basic biochemistry of the Wassermann, it does show sufficiently consistent relationships with other events to provide a basis for sound diagnostic inference. In short, it is a valid test. This is not true of the Szondi.

Yet, despite the uncritical nature of the exposition, the book has merit as an introduction to current formal psychodiagnostic practice. It is quite readable and should at least, as Alan Gregg states in the intro-

duction, "remove the burrs of misunderstanding and ignorance." Once these burrs are removed, some medical scientists can be depended upon to raise pertinent questions about the validity of specific procedures.

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A Further Study of Visual Perception. M. D. Vernon. New York: Cambridge Univ. Press, 1952. 289 pp. Illus. \$7.00.

In 1937 the author of this volume published its predecessor, *Visual Perception*. The earlier work was a rather brief, but nevertheless comprehensive, review of investigations throughout the entire area of its title. Critical comment was minimal, and no systematic position was adopted.

A Further Study of Visual Perception is a revision of *Visual Perception*. Only those earlier studies considered particularly important are now included in its coverage; for others, the reader is referred to such historical treatments as Boring's. A great many recent investigations find place in the volume, however, and thus its bibliography embraces over 500 references. There are 30 figures now, as against 19 in the original edition.

Once more the author ranges widely. After a brief discussion of "the nature of perception," she studies the phenomenology of the perceptual process, form perception, spatial perception, the constancies, frame-of-reference and anchoring effects, the time error, real and apparent movement, flicker, and fusion. Finally, there is emphasis on the recent work by Michotte (concerning the perception of causality, intentionality, and reality), and a rather long chapter on "internal and individual factors" in perception.

Theoretical integration is not stressed, although the author intersperses critical comments among experimental results. There is a general sympathy for the introspective, phenomenological approach. The stated thesis of the book is that the perceptual field is structured for stability by the perceiver, and that it displays an "extraordinary unlikeness . . . to the stimulus field;" a second point of emphasis is that there are large individual differences in perception.

It is a difficult task to summarize a great many investigations in a relatively brief volume—this the author has managed to do, and so provides a valuable reference book. Like most such books, however, it needs to be used with some caution. Where dubious methodology lies behind stated conclusions—as, for instance, in many of the motivational studies—that fact is not always indicated. There is perhaps a slight nativistic bias. Just occasionally, there are errors of fact: the figural aftereffect is inaccurately described, as is the Schafer-Murphy experiment; and Margaret Elizabeth Tresselt and Anna Gertrude Douglas are referred to by masculine pronouns.

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The Medical Sciences

Poliomyelitis. Papers and discussions presented at the Second International Poliomyelitis Conference. Philadelphia-London: Lippincott, 1952. 555 pp. Illus. \$7.50.

I have heard it said that international congresses on poliomyelitis (as on other scientific or medical problems) are a waste of time, because little or nothing is presented that has not already been published, or soon would have been published, or could not be read at leisure with greater profit. Since I have attended and greatly enjoyed both the 1948 and 1951 congresses on poliomyelitis, for reasons not solely related to the acquisition of new knowledge, I am perhaps not qualified to give a dispassionate opinion about their ultimate usefulness. It seems to me that these conferences serve their most useful purpose in providing a synthesis of accumulated, scattered information and, through the minds of certain gifted people, a critical evaluation as well. The individual publications on poliomyelitis are scattered in time and space in countless journals the world over, but within the covers of this book, containing the papers and discussions presented in Copenhagen in 1951, are many thoughtful communications dealing with almost every aspect of the numerous scientific and medical problems in poliomyelitis. The subjects covered range from the interaction of viruses and their host cells, the newer knowledge of the effects of poliomyelitis virus on various tissues *in vitro* and *in vivo*, the pathology, pathophysiology, immunity and resistance in invaded hosts, and the ecology of the infection, to the practical problems of laboratory and clinical diagnosis and the management of patients, with special emphasis on respiratory insufficiency, reconstructive surgery, physical medicine, and the social and psychologic aspects of the disease.

Those who are not impressed with the importance of holding these congresses and oppose them on the grounds of their great cost may suggest that the same synthesis could be achieved by publishing in book form, at three-year intervals, critical "present-status" reviews of the whole field. It must be pointed out, however, that the discussions presented at these conferences are as vital a part of the total synthesis as the formal presentations. Although the editors of the proceedings have not (at least in all instances) submitted the transcribed remarks to the speakers for correction, the informal discussions read quite well and, with a few exceptions, "sound" alive and spontaneous. The speakers should not, however, be held responsible for all details appearing under their names, for I have noted certain errors in the transcription of my own remarks.

Even those who attended the conference, but found it difficult to follow the on-the-spot translations of discussions in languages they did not understand, may find the English translations in this book informative

and intriguing. Thus, at the time of the conference, I was not actually aware of what the Russian delegates were reporting and was interested to read the remark attributed to Professor Levkovitch that, using monkey antipoliomyelitis sera, they had been successful in demonstrating specific complement-fixing antigens in the feces of "100 per cent of examined acute cases of poliomyelitis." If this can be confirmed, it would represent an advance in the rapid diagnosis of poliomyelitis that has not yet been achieved anywhere else in the world.

The value of this book is greatly enhanced by a complete reproduction (including photographs) of the many excellent scientific exhibits. It should be added that those who will consult this book in search of information on poliomyelitis will also be delighted to find a stimulating essay by Niels Bohr, entitled "Medical Research and Natural Philosophy," and penetrating remarks by Basil O'Connor on "Man's Responsibility in the Fight Against Disease."

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Pathologie Chimique, Vols. I and II. M. Polonovski, P. Boulanger, and G. Schapira, Eds. Paris: Masson, 1952. Vol. I: 832 pp., Vol. II: 770 pp. Illus.

The editors of this two-volume treatise, with the collaboration of numerous coauthors, drawn chiefly from the medical faculties of French universities, have provided a systematic and carefully organized presentation of chemistry in medicine. Polonovski indicates in the introduction, "*De prétention plus humble, mais d'ambition plus vaste, il voudrait initier le clinicien à une façon de penser biochimique et à une interprétation des symptômes morbides plus en relation avec le jeu des réactions cellulaires. . . Il n'y a plus de médecine hors la chimie!*" He expresses the hope that future editions will see the immense gaps of knowledge disappear. A 12-page table of contents evidences the care with which the material presented has been classified and organized.

Book One in Volume I, introduced by a concise résumé of present understanding of intermediary metabolism, includes a long section on water and the mineral elements and very brief comments on vitamins, hormones, and enzymes as related to deviations of metabolism.

Part One of Book Two considers pregnancy, normal and pathological, the fetus, the newborn, the nursing infant, puberty, and senescence. A brief commentary on the relation of food and eating practices to health opens Part Two. This more extensive section includes long chapters on the avitaminoses and cancer, and others on the biochemical effects of work and fatigue, climate, radiation, ultrasonic vibrations, burns, and viruses, and on the biochemistry of stress reactions, intoxications and detoxifications, shock, infections, immunity, allergy, and parasitoses.

Part One of Book Three, on the diseases of metabolism, contains chapters dealing with acid-base balance, cystinuria, alkaptonuria, and other anomalies in the metabolism of the amino acids, porphyrinuria and related conditions, gout, undernutrition and emaciation, obesity, steatosis, amyloid disease, polycoria (glycogen disease), hyperglycemias, and meliturias.

Volume II continues Book Three on the biochemistry of diseases. Part Two discusses at length diseases of the endocrine glands and includes a brief chapter on the thymus body. Part Three deals with the autonomic nervous system; Part Four, with the blood and reticuloendothelial system, including chapters on the lipoidoses and multiple myeloma.

Digestion, normal and pathological, opens Part Five on the diseases of the organs. Other chapters consider hypertension and abnormalities of the liver, heart, circulation, lungs, and kidney from the biochemical viewpoint. The final section deals with the pathological biochemistry of the nervous system, the muscles, joints, bones, teeth, skin, and the sense organs.

The text is clear and authoritative. The editors have competently avoided the space-consuming overlapping found at times in works of collaboration. Duplication observed is essentially only that resulting from the impossibility of separating an extensive field of knowledge into rigidly mutually exclusive categories. There is adequate use of structural formulas. Each chapter is accompanied by a list of references. In most instances these are brief, with heavy emphasis on secondary sources, books, and reviews. There is no attempt to provide exhaustive bibliographies. A lengthy index, with boldface type indicating the more important references is commendable.

The objective of providing the clinician with an up-to-date extensive and intensive survey of the implications of biochemistry for medicine has been well realized. It may be hoped that, as frequently as practicable, subsequent editions will be forthcoming.

RALPH C. CORLEY

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Chemical Induction of Cancer. George Wolf. Cambridge, Mass.: Harvard Univ. Press, 1952. 250 pp. Illus. \$3.50.

Most scientists work in compartments created by the nomenclature of their specialties. The modern babel of science is one of the barriers to progress in cancer research—a field that cuts across so many disciplines. The chemist often leaves his neighbor scientist bewildered when he presents organic formulas for concepts where no concise expression of words suffices. It is particularly noteworthy, therefore, when one writes so well—as Dr. Wolf has done—for the understanding of those outside his field of specialization. Although the book is addressed primarily to the medical reader, anyone who is interested in cancer research will profit by reading this review of the present state of knowledge of the chemistry and metabolism of the

chemical entities that produce experimental cancer.

In the first chapter the author discusses the relationship between organic chemical structure and carcinogenic activity of hydrocarbons. Beginning with elementary organic chemistry, he progresses through a brief development of the subject to an exposition of the complex polycyclic structures which are carcinogenic. Although the reader probably will not recover his knowledge of organic chemistry so casually, he should be able to regain an over-all impression that will be sustained through succeeding pages that are extensively illustrated with formulas of the compounds under discussion. This is not, however, simply a book of organic chemical considerations. In the next two chapters, the author turns about and discusses the "Biological Aspects of Tumor Induction" and "Test Methods" in words that are meaningful to the organic chemist. In reviewing carcinogenic hydrocarbons, azo dyes, and aromatic amines, Wolf has made many interesting evaluations of the data. His interpretation of the problem is set forth in the introduction, to the effect that "cancer research is concerned with the discovery of a cure for cancer," but the development of his philosophy of cancer research reveals a broad approach to the subject. Many readers will agree with the author that the highlights of the book are found in chapters on "Theories of the Mode of Action of Carcinogens" and "Biogenetic Relationships." The treatment of these subjects is speculative in character, but the circumstantial nature of the evidence and the tentative nature of the conclusions are never lost to sight. Readers are certain to be stimulated to new experimentation by these concepts. After each chapter there are bibliographical references to more technical reviews, but these are superfluous to an understanding of the book.

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The Literature on Streptomycin, 1944-1952. 2nd ed. Selman A. Waksman. New Brunswick, N. J.: Rutgers Univ. Press, 1952. 553 pp. \$5.00.

The present volume is not a critical survey or digest of streptomycin literature but is a revised edition of a smaller book issued in 1948. There is a selected list of references on actinomycetis (12 general and 12 on antagonistic properties) and on streptothricin (19 references). The greater portion of the book consists of the more or less chronological listing of 5550 references to the literature concerned largely with the medical uses of streptomycin, alone and in combination with other drugs. This listing covers papers published up to April 1, 1952. The remaining 50 references are to a few of the more important papers received after the closing date and are given only in the subject index. An author index (67 pp.) and a subject index (44 pp.) complete the book.

Since the original references are not grouped according to subject matter, the reader will rely pri-

marily upon the subject index in order to locate the references pertaining to a particular subject. A general survey indicates that the index could have been enlarged so as to increase the usefulness of the volume. The author is aware of the problem, for he states that "a special effort has been made . . . to provide a more detailed subject index." No doubt the difficulties are partly inherent in the fact that "in the great majority of cases, the original papers could not be consulted, and only the titles or brief abstracts were available." This situation is understandable when one considers the flood of papers published on streptomycin since the announcement of its isolation in 1944.

Some typographical errors were noted, but these do not detract from the general usefulness of the work. This volume will be of value to those primarily interested in the therapeutic uses of streptomycin, although references to the chemistry, production, and methods of assay and analysis are also included. Much labor has gone into this revised edition, and the compiler and his associates deserve praise for their efforts.

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A Survey of the Literature of Dental Caries. Pub. No. 225. Prepared for the Food and Nutrition Board, National Research Council, under the supervision of the Committee on Dental Health, P. C. Jeans, Chairman; Guttorm Toverud *et al.* Washington, D. C.: National Research Council, 1952. 567 pp. Illus. \$3.00.

Five coauthors have made an extensive survey of the literature on dental caries published before 1950 as evidenced by an excellent 58-page bibliography containing over 2000 references. The work is divided into 7 sections. The first is an introductory review, by Guttorm Toverud, of the literature concerning the relation of food and microorganisms to dental caries and the frequency of caries in various ancient, primitive, and modern civilized populations.

The second section, by Gerald Cox, reviews many studies that have been made on the experimental production of caries in animals. The types of tooth cavities that develop, the diets used, the bacteria involved, and salivary influences are included. The prevalence of dental caries is surveyed in the third section, by Sidney B. Finn. The data considered include race, age, sex, and geographic distribution. The need for statistical methods and more uniformity in examination procedures is stressed.

Section four, by Charles F. Bodecker, considers the pathology of dental caries and the histology and physiology of the teeth. Those publications that give the impression that metabolism in enamel is an essential factor in cavity production are stressed.

The complex problem of the oral environment is reviewed by Gerald Cox. The role of specific microorganisms, the physical, chemical, and antibacterial

influences of saliva on dental caries, as well as carbohydrate degradation and tests for caries activity, are appraised. The sixth section, also by Gerald Cox, contains a thorough review of the discovery of fluorine in biological tissues and fluids, the relation of this element to dental caries, and its use to reduce caries prevalence.

In the final section James H. Shaw reviews the extensive studies on nutrition. He includes surveys of the relation of diet to caries in various population groups, the possible influence of pregnancy, and the etiologic and preventive role of various nutritional factors.

The literature reviewed in each of these sections is classified into well-defined subsections, and the material is arranged in historical sequence. Brief, helpful summaries and conclusions expressing the author's opinion of the evidence reviewed usually follow each subsection. They express biased points of view and indicate an effort to reanimate the vital theory of dental caries, which holds that caries is dependent upon metabolic changes within the tooth.

Some scientists seem to believe that dental caries cannot be explained on a biological basis unless meta-

bolic changes occur within the tooth substance. Too often, the contributors to this book discount or overlook simple explanations, in the false belief that science must be complex. It is obvious that there must be a biological basis for caries because the process stops after death, and teeth, even the carious ones, become the longest preserved anatomic structures. On the other hand, caries has not been found in completely unerupted teeth. The oral environment of the tooth can be influenced as much or more by metabolic activity than the tooth substance itself. It is entirely sound for scientists to reason on the basis of available evidence that dental caries is primarily a local or environmental process. After all, the disease does begin on the exterior surface of the tooth. The *Survey* provides an exceptionally comprehensive source of information for students and investigators concerned with dental caries. It is unfortunate, however, that the treatment is biased in favor of one school of thought.

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Comments and Communications

How Much?

WITH the appearance of more and more foreign books in this country, there will always be the question, "How much does it cost?" Since a price is established for the book, that cost should be printed and given in dollars. Book reviewers should not be put in the position of reporting price in d., s., lire, fr., etc., only, nor should they be expected to check on foreign exchange. In one case I gave the foreign price and the approximate dollar value. That was a mistake, for other costs would naturally bring the price up. It is the responsibility of the distributor to give the dollar price when a book is listed in *SCIENCE*, *Chemical and Engineering News*, or any other publication.

One cannot help but be reminded of the old saw of that dear old lady who asked, "What is the charge on this battery?" When told "6 volts," she rejoined, "How much is that in American money?"

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Can Wind Move Rocks on Racetrack Playa?

McALLISTER and Agnew (1) touched off a series of articles, some popular and some merely amplifying the original observations, concerning the origin of the unusual skid marks on Racetrack Playa, Calif. Here, at an elevation of about 3700 ft in the northern Panamint Range west of Death Valley, is a playa 2.5 mi long and 1.5 mi wide, with a flat, mud-cracked surface on which are found numerous trails, many of them ending at isolated rock fragments hundreds of yards from the "shore." The tracks contrast with the general playa surface in being depressed a fraction of an inch and lacking most of the almost paper-thin curled flakes of dried mud that are usually found atop each mud-crack polygon. The fact that the widths of these tracks are nicely proportioned to the sizes of the rocks at their ends, under the leading edges of which the missing mud flakes are sometimes packed, leaves little doubt that the rocks made the tracks and that, where no rock is now present at the end of a given trail, it has either been removed by souvenir hunters, or its role was taken by some less durable object (1; 2, Fig. 5). The trails are oriented in different directions, and many are composed of straight or slightly curved segments compounded into jagged patterns (2). A few have kinks or loops. The maximum measured length of a single trail is known to exceed 700 ft (2).

In April and May, 1952, three visits were made to Racetrack Playa in light aircraft for the special purpose of discovering whether any stones could be moved with the artificial wind velocities produced by the propeller wash of the planes. Equipment included 25

gal water, laboratory-cut cubes of limestone in graduated sizes 0.5-3 in. on an edge, spring balances, and an anemometer.

It was found that even 3-4 hr soaking is not enough to produce a smooth slippery film of mud on the playa surface; probably days (or weeks) are required to close the mud cracks. Accordingly, water confined to an area of about 4 sq ft was worked into the surface with a trowel until a slippery paste was formed. Whether its consistency or depth approached natural conditions is not known. However, at air velocities not exceeding 42.2 mph, a natural triangular prism of limestone picked up from a nearby track and weighing 19 oz was made to slide part way across the wetted area. None of the prepared cubes slid (though the smaller ones showed some tendency to roll over), probably because of the greater pressure per unit area of their bottom surfaces and, perhaps, frictional retardation of the "wind" close to the playa surface.

Slightly better results were achieved at a shallow, man-made waterhole near the south end of the playa. By draining some of the water to one side, a larger area of thoroughly saturated playa material (not entirely a present-day surface layer, however) was exposed, and on this two natural stones, including the 19-oz limestone mentioned above, moved several times over distances up to about 2 ft under measured wind velocities of 40-45.5 mph. This was recorded on moving picture film. Movement was slow and sometimes halting, and the mud surface was actively rippled by the air blast.

The wetted surfaces used may not have simulated natural conditions, and under the conditions of the experiments any tracks left might have been blurred or destroyed by further action of the wind. The mud used was probably less slippery than would be that composed of the fines from the uppermost $\frac{1}{8}$ in. of the playa surface, and the wind velocities attained were low compared to the gusts that undoubtedly occur under natural conditions. It should be pointed out that the windward (west) side of the playa is paralleled by a steep narrow ridge rising over 1500 ft above its surface (see USGS Topographic Map of the Ballarat Quadrangle) the abrupt ends of which, especially at the south, nearly coincide with the north and south ends of the playa. West of this ridge is the 10 x 20 mi expanse of Saline Valley, with a north-west-trending floor 1500 ft lower than Racetrack Playa. This topographic situation favors crowding of flowlines over the southeast rim of Saline Valley, and turbulence around the ends of the barrier ridge may account both for peak gusts of high velocity and the variable direction of the tracks.

The presence of mud flakes in front of some rocks suggests that movement took place (at least in these cases) while the flakes were still discrete, owing either to lower permeability or to their being held together

by algal filaments. In other cases the optimum conditions might be (as suggested to me by Robert P. Sharp) those existing when the playa has been well soaked, frozen, and has just begun to thaw, thus yielding a thin surface layer of thoroughly saturated mud supported by frozen ground below.

Last, it should be reported that a tissue paper-like film was peeled off some of the tracks, especially at spots where it looked as though the rock had stopped for awhile (and in many instances had also changed direction). In the laboratory J. D. Lauder milk was able to revive this "pond paper," and he reports it is the blue-green alga *Microcoleus* sp. Similar fibers permeate the curled mud flakes occurring outside the tracks. *Microcoleus* filaments are described as having a "... homogeneous sheath of an extremely gelatinous nature" (3). Perhaps this material is an important Department of Geology, Pomona College lubricant.

JOHN S. SHELTON

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S-Acetyl Pantetheine (Acetyl LBF)¹

THE importance of acetyl coenzyme A (1-3) (acetyl CoA) as a biological acetylating agent, together with the description of other S-acetyl compounds (4, 5) possessing acetylating power for such acceptors as hydroxamic acid, has prompted us to attempt the synthesis of acetyl pantetheine (acetyl LBF), which may be viewed as a model compound of acetyl CoA. S-acetylthiophenol ($C_6H_5SCOCH_3$), which was employed by Wieland and Bakelmann for the preparation of acetyl glutathione (5), was found satisfactory for the synthesis of acetyl LBF.

Pantetheine was prepared through condensation of β -aletheine (N- β -alanyl-2-aminoethanethiol) with (-)-pantoyl lactone (6). A mixture of 12.9 g acetylthiophenol in 30 ml methanol and 5 ml water was adjusted to pH 3.0 with hydrochloric acid. The solution was added to 2.4 g of freshly prepared pantetheine (kept in the reduced state under nitrogen) in a methanol solution. The homogeneous mixture was allowed to stand at room temperature for 5 hr under nitrogen. The solvent was then distilled off *in vacuo*. Excess thiophenol was removed by three successive ether extractions. The residue after extraction was dried overnight in a vacuum desiccator at 0.01 mm pressure. Yield, 2.0 g of pale yellow oil. The active acetyl content corresponded to the bound pantothenic acid, as shown in Table 1. These data

¹ Paper No. 12 of a series on pantothenic acid studies. This work has been supported by the Nutrition Foundation, Inc., the Eli Lilly Laboratories, Inc., the Abbott Laboratories, Inc., and the Office of Naval Research, Contract NR 123-058. Published with the approval of the Monographs Publications Committee, Oregon State College, Research Paper No. 213, School of Science, Department of Chemistry.

TABLE 1
THE ACETYL AND PANTOTHENIC ACID CONTENT
OF ACETYL LBF*

	Acetyl (7) (μ mols)	Bound pantothenic acid (8) (μ mols)	Molar ratio of pantothenic acid to acetyl
Found	178	180	1.01
Theoretical	189	189	1.00

* 60.4 mg of the product was dissolved in water for analyses.

revealed that the product was about 95% pure. It readily acetylated hydroxamic acid. Acetyl LBF was at least as active as LBF in supporting the growth of *Lactobacillus bulgaricus*. This was presumably due to hydrolysis to LBF. Acetyl LBF gave a positive nitroprusside test (in NaCN and concentrated NH_4OH) slowly upon standing, in contrast to the immediate reaction produced by LBF.

By the same method acetyl CoA in purity of about 60% was obtained from a sample of CoA, 75% pure. The further study of the preparation, as well as the biological behavior of acetyl LBF and acetyl CoA, is in progress.²

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² J. Baddiley and E. M. Thain have prepared acetyl LBF through reaction of the sodium salt of pantetheine with acetyl chloride. (See communication following.)

It has been shown that certain thiolacetates acetylate amines under very mild conditions (1). As this was of interest in connection with the mode of action of coenzyme A, the compounds examined were acetyl derivatives of 2-mercapto-ethylamine and its β -alanyl amide. This series has now been extended to include S-acetyl pantetheine, the synthesis of which is described here.

When a solution of pantetheine in methanol was treated with 1 M sodium methoxide followed by exhaustive removal of solvent *in vacuo*, an S-sodio derivative was obtained. To a suspension of this in anhydrous dioxan was added 1 M acetyl chloride with vigorous shaking at room temperature. After stand-

ing for $\frac{1}{2}$ hr the solvent was removed *in vacuo*, the residue dissolved in chloroform and filtered. Evaporation of solvent left a resin consisting of almost pure S-acetylpantetheine. (Found: C, 47.8; H, 7.5; N, 8.5. $C_{13}H_{24}O_5N_2S$ requires C, 48.6; H, 7.5; N, 8.7%.) It was shown to be homogeneous by paper chromatography in the following solvents: butanol-water (R_F 0.73), amyl alcohol-water (R_F 0.70), butanol-acetic acid-water (R_F 0.82). The product was demonstrated on paper by spraying with ammonia solution prior to the cyanide-nitroprusside spray.

This material was indistinguishable from a sample of S-acetylpantetheine synthesized in a different way by King, Stewart, and Cheldelin (2). It acetylated hydroxylamine with great rapidity at room temperature in dilute solution.

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Pleistocene Corals at Lake Worth, Florida

A COLLECTION of corals believed to be of Pleistocene age has been obtained in Lake Worth, Fla., some 60 miles north of Miami, the northern limit of the living reefs. The coral-bearing zone is buried under sand at a depth of 30 or 40 feet above sea level. Fragments of corals were first found in January 1950 by John H. Irons, of Lake Worth, while he was hunting shells on the spoil bank made by a large suction dredge operating near the western shore of Lake Worth, about half a mile north of the bridge connecting the town of Lake Worth with Palm Beach. He has kindly agreed to my publishing this note.

Mr. Irons' interest was aroused because the corals were thoroughly impregnated with calcite or aragonite, a mode of preservation quite different from that of the geologically younger mollusks on the spoil bank. He has continued to search systematically for fossil corals and has accumulated several hundred specimens, which range from small heads to masses weighing more than 100 pounds. Although the corals are recrystallized, the details of structure of many are plainly visible through the clear crystal, and the beauty of the specimens is thereby enhanced.

A representative assortment of corals from the Irons collection was examined by John W. Wells, of Cornell University, a specialist on corals. It included ten species representing eight genera, and several additional species have since come to light. Nearly all the species identified by Dr. Wells are common in the Pleistocene and living reefs of the Florida Keys and the West Indies. The Lake Worth corals are particularly interesting because of their occurrence north of their present range in Florida. No comparable Pleistocene reef has previously been discovered north of the keys, which are themselves based on a massive Pleistocene reef, the Key Largo limestone.

That the fossil corals at Lake Worth are Pleistocene seems obvious, but to determine to which part of the Pleistocene they should be referred needs further consideration. The occurrence is about half a mile west of the Palm Beach peninsula and about 50 feet below its summit. This peninsula appears to have accumulated as an offshore bar during Pamlico time, when sea level stood 25' higher than now. The corals are probably older than the Pamlico.

A further indication of antiquity is the fact that the corals are buried under 20' of sand. The lower part of this sand may be contemporaneous with the bar, but the upper part was probably deposited in Silver Bluff time, for the dredged area lies just east of the Silver Bluff shoreline. Silver Bluff and Pamlico time together are believed to span the time of deposition of the Peorian loess.

The corals could not have lived during the preceding Illinoian (third) glacial stage, for the area was then presumably dry land. Moreover, the sea water at this latitude was then probably too cold for these tropical corals. During the Yarmouth (second) interglacial stage conditions would have been more favorable for the growth of corals. All of southern Florida was then submerged, and the water over the site where the corals grew may have been as deep as 170' at the beginning of the Yarmouth, or as shallow as 70' near its close. This range of depth is quite suitable for corals, though rather deep for massive reefs (1). The Yarmouth, then, seems to be the latest time for the growth of corals at Lake Worth.

In terms of coastal-terrace chronology, the Yarmouth is supposed to comprise the interval from the formation of the Okefenokee terrace (sea level about 140' above the present) to the Talbot (sea level 42'), the intervening steps being the Wicomico at 100' and the Penholoway at 70' (2).

The Lake Worth corals probably form part of the limestone composing the "rim of the Everglades," which must be older than the Pamlico, because it supports (at West Palm Beach) a sand bar of the Talbot formation. This limestone has been interpreted (3) as a facies of the Anastasia formation, which consists typically of coquina. The corals may be contemporaneous with the deposition of the Key Largo limestone, which is the southern extension of the "rim of the Everglades" and which has been correlated with the Anastasia. The coral heads may have been firmly cemented with other organisms into a massive reef, but this is not certain, because the entire collection was recovered from dredgings.

C. WYTHE COOKE

U. S. Geological Survey, Washington, D. C.

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Evolution in the Genus *Drosophila*

by John T. Patterson and Wilson S. Stone

The most extensive treatment of any living genus thus far attempted, the book covers the evolution of the *Drosophila* as demonstrated by living forms. It is mainly concerned with isolating mechanisms, genetic systems shown by hybrids, and the cytology of over one-third of the known species of the genus. Being a monograph of the Genus *Drosophila*, the book gives the distributional relationships, chromosomal evolution, and changes in genetic systems leading to isolation which allows the genus to diversify.

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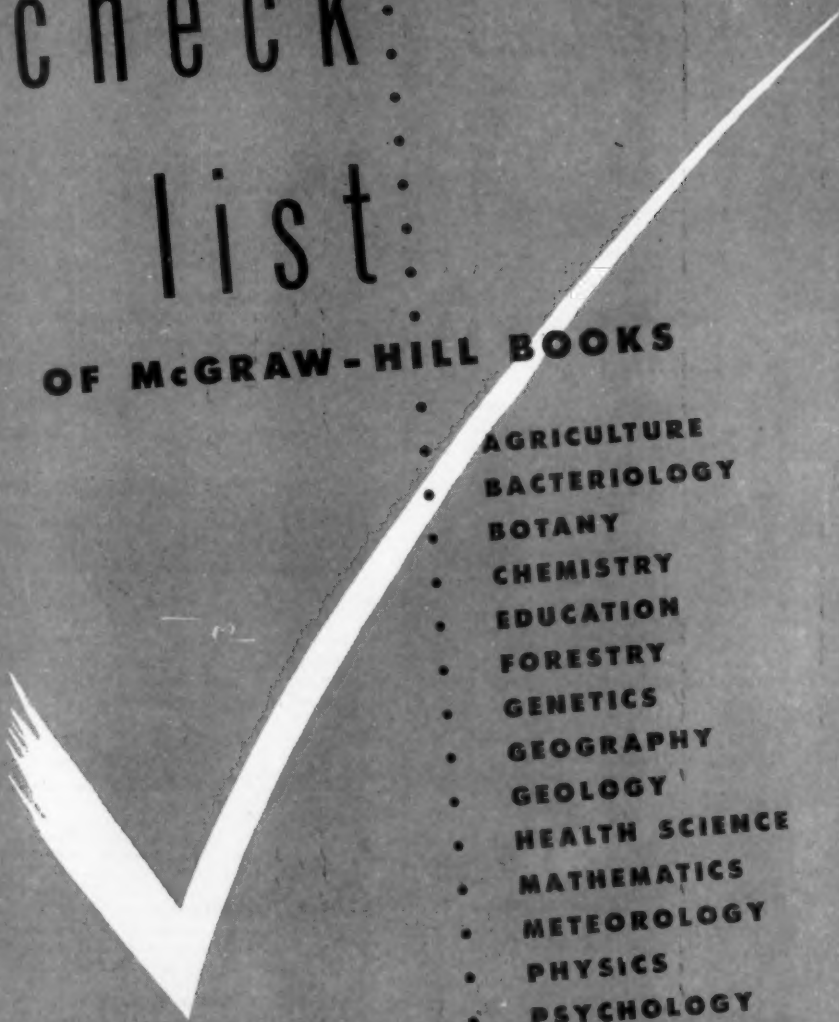


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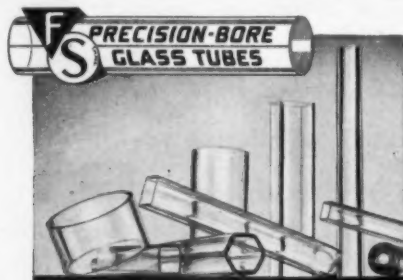
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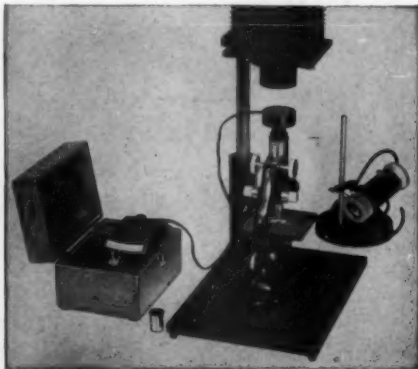
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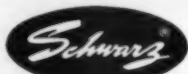
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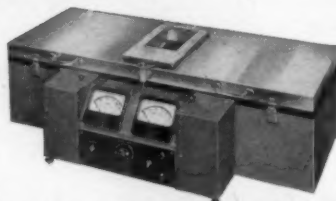
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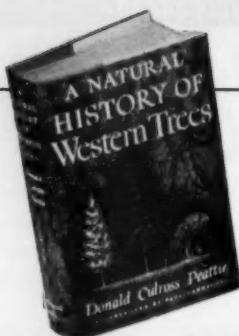
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